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James Lind: His Contributions to Shipboard Sanitation

A. EDWARD A. HUDSON* AND ARTHUR HERBERT†

THE early eighteenth century saw the birth of such scientific and medical giants as William Cullen, William Heberden, John Hunter, Carolus Linnaeus, and one James Lind. Of these the name of Lind will always be remembered for the very important rôle he played in nautical medicine, through his recommendations for the betterment of sanitary conditions aboard ship and numerous suggestions which finally resulted in a concentrated attack upon the sea diseases, to the benefit of the sailors of all nations.

The sea diseases, as scurvy, fevers, and fluxes were called, first appeared on a large scale in the sixteenth century, and continued for the next two hundred years, waging death on the vessels sailing the high seas. It took until the end of the nineteenth century to check them.

Little information is available regarding Lind's early life. He was born in Edinburgh in 1716, in the century known as the heyday of the sea diseases. He entered the Royal Naval Medical Service as a surgeon's mate in 1739, at twenty-three years of age, spending nine years at sea during which time he rose to the rank of surgeon. He served with the Channel Fleet, but the major portion of his seafaring life was spent in voyages to the Guiana Coast and to the West Indies.[1,2,3]

In Lind's day sailors aboard ship lived in overcrowded, damp, dark, airless, foul-smelling quarters, sleeping in dirty bedding, and working in the same filthy vermin-infested rags in which they came aboard. The food on the long voyage was of the poorest, Lind wrote, "consisting of putrid beef, rancid pork, mouldy biscuits and flour . . . the number of seamen in time of war who die by shipwreck, capture, famine, fire and sword is but inconsiderable in respect of such as are destroyed by the ship diseases and by the usual maladies of intemperate climates."

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James Lind, M.D. (1716-1794).
Pen drawing by A. E. A. H. from a portrait by Sir George Chalmers.

The lot of the surgeon's mate in the ships of the Royal Navy up to the end of the eighteenth century was by no means an easy one. He wore no special uniform, holding only warrant rank, and while his name appeared upon the ship's roster along with those of the carpenter and gunner, he received less pay than either of these ratings.[2,3] His living quarters were usually in the cockpit on

the orlop deck just above the hold, the space allotted being little more than six feet square, into which was crowded a medicine chest and a board which served as a table. This cell was enclosed with a canvas, which was nailed to the beams of the ship, to serve as protection from the cold and view of the crew.

To the surgeon's mate fell the most arduous duties and menial tasks. He would boil the gruel, barley water, and fomentations, wash the towels and dressings, prepare and change dressings, mix and spread plasters, fill and carry the patients' water buckets, sweep out the sick bay, and often be required to empty the buckets used as commodes. He was at the beck and call of the ship's surgeon twenty-four hours a day.

The day's work commenced with mustering the sick, who received their summons through a boy who went the round of the mess decks, ringing a hand bell, and chanting "rhymes composed for the occasion, inviting all those who had sores to repair before the mast, where one of the doctor's mates attended, with applications to dress them." In the evening, after supper, the surgeon's mate went the round of the ship, visiting the sick in quarters and in the sick bay. Here the patients lay in hammocks, suspended in rows, huddled one upon another, for as little as fourteen inches of space was allotted for each bed. Into these quarters little or no light of day entered, "each patient breathing the stagnant atmosphere of the morbid air, exhaling from their own excrements and diseased bodies."[2,4,6]

A lurid account of the hardships of the life of a surgeon's mate in the Royal Navy is given by Tobias Smollett in his great novel, *Roderick Random,* published in 1748. Smollett entered the Royal Naval Medical Service in 1739, the same year that Lind entered the navy, and was assigned to *H.M.S. Cumberland.* In *Roderick Random,* Smollett immortalized his own experiences while serving afloat and as a member of the Cartagena Expedition (1741-1743).[5,5,7] While Smollett has been accused of considerable exaggeration in his portrayal of both the life of the seaman and the treatment of the sick in the Royal Navy during that period, his novel did much to attract the attention of the public to the vile conditions that existed on the ships during long voyages. As can well be imagined, such a publication could not have been popular with the naval authorities, for if any man had any doubts at all about selecting a sailor's life, he certainly had none after reading this novel.

Such was the life that Lind was in large part exposed to for nine years; then in 1748, at the age of thirty-two, he returned to the city of his birth, where he obtained his M.D. degree in the same year at the University of Edinburgh.

During the next decade Lind divided his time between the practice of medicine and working on the vast amount of material he had gathered during his days at sea. He had two objectives in view: one was to bring to the attention of the naval authorities a picture of the poor hygienic conditions aboard ships with recommendations for their improvement, and the other to make suggestions to the medical profession by which more attention would be given to the care of the sick at sea and more stress be placed upon the prevention of the sea diseases such as scurvy, dysentery, and typhus fever.

In 1754 Lind presented his first paper,[7] *A Treatise on the Scurvy,* which marked an epoch and is too well known for consideration here. In 1757 his second work, *An Essay on the Most Effectual Means of Preserving the Health of Seamen in the Royal Navy,* appeared and brought him fame. This publication went into three editions, the third edition being published by authority of the Lord's Commissioners of the Admiralty; in this Lind is described as "Physician to His Majesty's Royal Hospital at Haslar, near Portsmouth." The book was translated into French and Dutch.[8] And there is no doubt that it had much to do with Lind being appointed as physician to the Royal Naval Hospital at Haslar in the year 1758. This hospital had a thousand beds and had been opened for the reception of patients only four years prior to Lind's appointment; the work of construction was not fully finished until 1762.

In 1758 Lind was elected to Fellowship in the College of Physicians of Edinburgh. While at Haslar he published in 1768 his third work, a manual on tropical diseases.[9] In the *Essay* are discussed medical and hygienic reforms of vast importance to the navy at that period; it revealed that Lind was years ahead of his time in preventive medicine.

The contents of his book may be considered under four sections: (i) Recommendations for the practice of hygiene aboard ship; (ii) comments upon diseases in Northern Latitudes especially the scurvy; (iii) advice on the prevention of tropical fevers; (iv) means of procuring a wholesome water supply at sea.

Lind vigorously attacked the condition of the men collected by the press gangs and the manner in which they were sent to the ships of the fleet. Due to the great hardships met with at sea, and the tremendous uncertainty of returning home again on account of disease contracted afloat, sea service was far from popular. "The freedom of England from attack and the security of the English people were due to the vigilence and activity of the Fleet, which could not, therefore, be allowed to become undermanned." To obtain the required number of men for the ships, various systems had been tried, such as the offering of bounty to volunteers, the quota system, in which each county was required to send a fixed number of men a year, and registration, all of which failed to produce the needed seamen. The only certain way to obtain men, then, was to take them by force, such method being known as impressment.

The press gangs roamed the docks and the streets of all the English seaport towns and even the villages, picking up any man that was unfortunate enough to cross their path. Often the head of a family on his way home from work would suddenly disappear, never to be heard from again. In many instances, not only was a family left in total ignorance of the whereabouts of the wage earner but without any means of support from then on. As Scott has so aptly said, "It was a curious anomaly; the security of the citizens depended on the Fleet, the manning of the Fleet was, therefore, a prime necessity, and the citizens—the prested men among them, at least—were 'made slaves' in order to keep them free."[1]

During the eighteenth century so great was the demand for seamen to man the ships of the Royal Navy that applications were made to local magistrates for their prisoners. Many of these men, who accepted service more or less gladly to escape the harsh sentences in effect at that period, were already infected with jail fever (typhus) or dysentery. Tramps and vagrants in the poorest physical condition, clad in filthy verminous rags, were picked up and confined for considerable time in a narrow pen. Often as many as fifteen men were placed in a 'press room' not over ten feet square, later to be sent to a common guardship along with healthy men. Crews from merchant ships were also taken, often from incoming ships so that they were already infected with some tropical disease or on the verge of scurvy. From the guardship the men were sent to the ships of the Fleet to spread the fevers to a healthy crew.

Lind drew attention to the fact that such a system did much to increase the incidence of disease and that the most common cause of death among the pressed men was typhus fever, then called "ship" or "jail" fever. Common practice, he said, was to hold all pressed men aboard a poorly kept guardship until assigned to their ships; while there they had no change of clothes, remaining in the same dirty rags they wore when picked up by the press gang. These individuals died either while on the guardship or later when transferred to other ships. Upon these facts Lind based his first suggestions for reform. He denied the theory that the fevers were caused altogether by not keeping the vessels clean. "No," he said, "this fever was caused and spread by the infected clothing of these pressed men, such mischief lurked in their tainted apparel and rags, and by these were conveyed into other ships." By these statements Lind was thus the first to give heed to the importance of bodily cleanliness in the prevention of typhus fever. It is remarkable that he did not consider the part the body louse might have played in the transmission of this disease when, in short, he recommended 'delousing.' He erroneously stated that the contagion was not "propagated by animalculae."

The following are Lind's instructions for the prevention of typhus fever:

> The most effectual preservative against this infection, during a press, would be to appoint a ship for receiving all ragged and suspected persons, before they are permitted into the receiving guardship. This ship should be furnished with slops, shirts, bedding, and all the necessary articles of seamens apparel; with soap, tubs, and proper conveniences for bathing, and with a room upon deck for fumigating of clothes. Every suspected person, whether impressed at sea, or on shore, should first be put on board her; their stay on her, however, should be short, as soon as they are stripped of their rags, well washed and cleaned, they should be supplied with new clothes and bedding, and be sent on board the receiving guardship. Such of their apparel as appears tolerably good ought to be cleaned, or, if necessary, fumigated with brimstone and returned to them; but it will be absolutely necessary to destroy all filthy rags, and all such clothes as are brought from Newgate and other prisons.

These recommendations of Lind for the prevention of typhus fever aboard ship are in substance still good today.

In further comments on the subject of clothing Lind suggested that it would be well for the navy to place the seamen in a standard type of uniform as well as "some little movable badges, or variations (if judged necessary) by which it might be known to what ship they belong." One hundred years later this was done.

On the subject of naval hygiene Lind wrote that it would be desirable to set up the seamen's hammocks on deck every morning for airing while the mess decks were washed down and the gun ports opened. The decks, he said, should be washed in the morning and not left to be washed after sunset, as was the habit, such being a "preposterous custom." In bad weather the dampness on the decks was to be overcome as much as possible by placing above them coal fires in suspended buckets. He stressed the scrubbing and cleansing of every hammock in the ship at least once a month.[8]

Drawing attention to the poor condition of the food served in the ships of the navy, Lind emphasized the fact that the British made far less effort to provide broths and other fresh meat soups for their sick than did the French or Spanish. He deplored the unsanitary and poor conditions existing in the sick berths in British ships, condemning the practice of the use of the hold and upper gun deck for the housing of patients, the former having little ventilation and the latter being too damp. The best place to house or confine the sick, he said, during warm weather or in the tropics was under the forecastle, while in bad weather or in cold climates the gun room should be used.

Lind had much to say about the error of sending sick seamen ashore for treatment without sending prior notice to the hospital officials of the type of case that was involved, for only by such notice could the spread of disease in the wards be prevented. He complained that many times the first notice of the nature of the diseases was upon the arrival of the patients at the hospital, it then being too late to separate the noninfectious patients from the infectious ones. To prevent such situations he suggested that "when a ship arrives from a cruise or voyage having either a malignant or spotted fever, the flux, or any other disease on board, which is plainly contagious, that, in this case, the captain or surgeon should acquaint the physician or surgeon of the hospital with their condition, previous to the landing of the sick, that proper and distinct wards may be prepared for their reception."

Under the heading of prevention of sickness in northern latitudes Lind proposed methods to prevent the bronchial, rheumatic, pleuritic, and "peri-pneumonic" ailments which affected so many of the seamen.[8] He said, "Dry, warm clothing and bedding are of the greatest consequence in winter, and without which, other means of preserving health will have very little effect."

Lind was of the opinion that most of the acute inflammatory chest conditions came from a stoppage of the perspiration and that the remedy was to be found in re-establishing skin function, plus the promotion of catharsis and diuresis. For those suffering from chill and fatigue after exposure to heavy gales and storms he prescribed a mixture in use by the Russians:

Small beer	1 pint
Brandy	1 pint

Molasses and vinegar to sweeten and acidulate, as desired.

While Lind had already discussed scurvy in his paper of 1754, he repeated much of the material in this essay, making it clear that he was convinced that in the prevention and cure of this disease, the essential elements of diet were fruit and vegetables.*

Under the section of tropical fevers, Lind discussed the advisability of gradually reducing the amount of salt provisions, such as salt pork and beef, given the crews of vessels sailing to tropical climates. He had much to say against the existing practice of allowing ships to depart for foreign regions with more than their full complement of men. This custom had been considered necessary because a large number of the men were expected to die while the ship was on its voyage to the foreign station, and by taking excess men it was thought that enough would remain alive to work the ship for completion of the passage. Lind stated that such a policy led to an increased number of deaths when no measures of hygiene were exercised.

To the subject of the cause and prevention of tropical fevers, Lind had evidently given much thought, distinguishing between the two fevers most commonly affecting sailors in tropical waters; "What is called the Yellow Fever or Black Vomit," and "a malig-

* Lind was by no means the first to prevent or cure scurvy by means of fresh fruit. The first British naval officer to use oranges or lemons for this purpose was a Sir Richard Hawkins,[10] in 1593, who wrote in 1622: "That which I have seen most fruitful for this sickness, is sower Oranges and Lemons." He further stated that "This is a wonderful secret of the power of God, that has hidden so great and unknown vertue in this fruit, to be a certaine remedie for this infirmitie." In 1601 a Captain John Lancaster, of the East India Company, stated that he had found lemons to be of much value in preventing scurvy.[11] Doctor John Woodall, past sea surgeon and Master of the Company of Barber Surgeons, in his book *The Surgeons Mate,* published in 1617, stated "the juyce of Lemons is precious medicine and well tried being sound and good."[12] Lind, in insisting that all ships proceeding on long voyages be well stocked with either the "rob" of lemons or the juice of oranges, referred to the experiences of the above to add weight to his recommendations. He had a novel idea for supplying fresh vegetables at sea, suggesting that leeks could be preserved in salt and if kept in well-covered wooden dishes, they would retain their antiscorbutic properties for as long as three months. Watercress, he said, could be grown in the hold of a ship by scattering the seeds over wet cotton or scattering them upon blankets which had been soaked with rain water.

nant fever of the remitting or intermitting, but most frequently of the Double Tertian Kind."

Lind accepted the prevailing thought that malaria was caused by bad air and as a remedy and preventive he had officially recommended cinchona bark. He prescribed for routine use a mixture consisting of "eight ounces of bark and four ounces of dried orange peel, infused in a gallon of spirits. . . . Two ounces of this composition, which will be but a very moderate dram to a sailor, may be allowed to each man a day upon the approach or apprehension of these malignant diseases." Men working inshore, he said, should be invariably fortified with this draught.

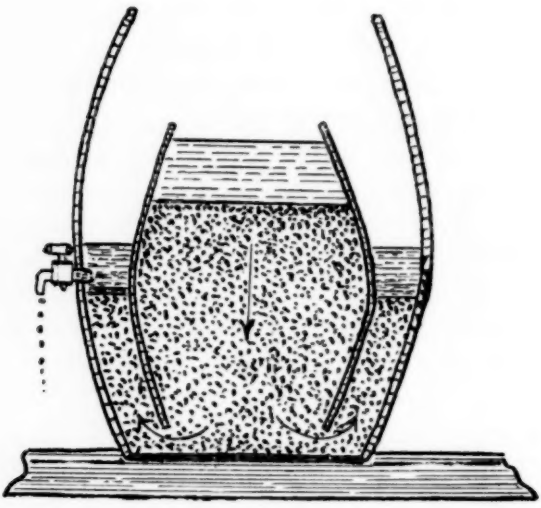
He also stated that if men were compelled to spend the night ashore they should light a fire "producing a little smoak, constantly burning in their hut where they sleep." Lind suggested this practice because he had seen that the West African natives always kept a fire burning at night while they slept upon the ground. It seems strange to us in this day that Lind, with his great imagination and keen powers of observation, should have overlooked the possible part that insects might take in transmission of disease, especially after stating that places situated near a morass or a river having low, muddy banks were dangerous, particularly when the air was full of insects and "animalculae." These were sure signs of "malignant disposition" of these places, he said.

Lind advised against the ships anchoring too close to shore when in tropical waters, recommending that "When ships are necessarily obliged to put into such unhealthy parts, the first precaution to be taken, is, to anchor at as great a distance from the shore as can well be done, and to prefer the open sea, where the anchorage is safe, to running up rivers or bays enclosed with the land." He was convinced that no danger existed from tropical fevers while a ship remained at sea. "The higher ships sail up these rivers upon the Coast of Guinea, the more sickly they become; such, however, as do keep two or three leagues at sea, beyond the reach of the land breeze, are, for the most part, healthy."

The fluxes or diarrheas were greatly feared by the ship's officers, one or two cases often resulting in an outbreak sweeping through an entire vessel. Lind was of the opinion that the drinking water played an important part in many of these cases. He was a pioneer in stressing the importance of pure drinking water being supplied to the ships and did much to bring this about by means of his recommendations in the essay. He wrote, "Bad water is next to

bad air, a frequent cause of sickness, especially of the flux, in places situated near the Torrid Zone." He advised that great care be taken by the water parties sent ashore to fill the casks, and if any doubt existed as to the purity of the water, that it should be filtered before used for drinking purposes.

He constructed an effective and simple piece of apparatus for the filtration of water. This consisted of two barrels; a small one, with both ends taken out; and a larger one from which the top



had been removed. Both barrels were partially filled with well cleaned sand or gravel, then placed one within the other. For filtration the water was poured into the smaller barrel, where it seeped down through the sand, to come up through the sand into the larger barrel, where it was removed from the surface by means of a tap fitted into the barrel's side.

For several years Lind had been interested in the distilling of fresh water from sea water. In 1762 he sent a Doctor Robertson, F.R.S., Master of the Royal Academy at Portsmouth, the description of an apparatus which utilized the sun's rays to boil the sea water. Lind stated that his method was superior to other devices that had been suggested.

In 1754 a Doctor Appelby had tried mixing sea water with calcined bone before distillation, thinking that this would retain the natural taste of the water. However, the method was unsatisfactory as the water was still too unpleasant to be used for drinking

purposes. Credit for the discovery of a suitable means for sea water distillation was given in 1776 to a French physician, Doctor Poisonniere, and to a surgeon of *H.M.S. Arrogant*, Doctor Irving. Lind stoutly maintained that he was the first to present a practical method of sea water distillation and that credit should have gone to him and not to Doctor Irving, stating, "ten years after my first publication of it Dr. Irving also, on the same account received the sum of £5000 from the British Parliament." It is of interest to note that the outcome of all this was that two years earlier a ship, *H.M.S. Dolphin*, made a voyage around the world, the water supply being entirely distilled water.

The authorities were very slow indeed in placing Lind's important recommendations into effect, paying more attention to the design of new ships and the manner of arming than they did to the health of the men who were to sail them. It was in 1781 that first official action was taken on his recommendations, when the Admiralty ordered that all the newly pressed men should be placed in a special receiving ship, where they could be washed and provided with clean clothing before being sent to other ships. Thus, a much needed and important advance was made in preventive medicine which did much to lower the incidence of typhus in the fleet. No official action was taken on the problem of scurvy until the year 1796 when the Admiralty made official the use of lemon juice for prevention of this disease.

In 1772 Captain Cook set sail for his second voyage around the world; an excellent navigator and able sailor, he was also an early convert to the teachings of Lind, holding to the same viewpoint on matters of naval hygiene. This great master mariner, in paying as much attention to the welfare of the health of his men as he did to the care of his ship, was able to end his voyage in 1775, without the loss of a single man from the sea diseases.[13] In this the teachings of Lind most certainly played an important part.

True, Lind overlooked the rôle played by the body louse in the transmission of typhus fever and the importance of the mosquito insofar as malaria was concerned; but his recommendations, which have proved themselves to be so sound in our time, entitle him to an honored place in the pages of public health history. The history of ships and those who sailed them has been well illustrated with accounts of the effects and hazards of battle, fire, mutiny, shipwreck, and storm, but too little has been said of the huge toll of sailors' lives taken by the sea diseases.

Lind achieved more lasting good than any physician of his period, for as a result of his professional ability, foresight, and keenness, ships from all nations were to carry men under better hygienic conditions, and be better protected from disease than they had been before his time. His lot, however, was like that of many other scientists before and since his time, for Lind received no honors in his lifetime, no election to Fellowship in the Royal Society, and no official commendation of his work by his government.

James Lind resigned from the Royal Naval Hospital at Haslar in 1793, after serving it well for twenty-five years, and was succeeded by his son, John Lind. He died at Gosport on 13 July 1794 and was buried at Portchester, a few miles from Haslar; the tablet in the church there, a posthumous honor, commends him to the attention of all men. His friend Doctor Thomas Trotter[14] had given him the title, "Father of nautical medicine," and we might perhaps be correct in adding, James Lind, "Pioneer in Public Health."

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The Invention of Eyeglasses

EDWARD ROSEN*

It is of importance to the sciences that the history of memorable inventions should be handed down correctly.

Gottfried Wilhelm Leibniz[1]

Much has been written, ranging from the valuable to the worthless, about the invention of eyeglasses; but when it is all summed up, the fact remains that the world has found lenses on its nose without knowing whom to thank.

Vasco Ronchi[2]

PART I

The Pisan Chronicle

THIS subject was first discussed[3] by Carlo Roberto Dati (1619-76) in a *Veglia* or essay entitled *The invention of eyeglasses, is it ancient or not; and when, where and by whom were they invented?* Dati belonged to the Accademia della Crusca,[4] a scholarly society of Florence, where he read this *Veglia*. Giving it the form of an imaginary conversation, he put into the mouth of an interlocutor the following speech:[5]

When I was quite young, I went to Pisa to study law, more by order of others than by my personal inclination, which turned toward the recovery of our own past records rather than toward Roman jurisprudence. I always used to examine the manuscript collections, where ordinarily one finds something erudite, not a matter of common knowledge; and particularly the collection at the Dominican Fathers' St. Catherine, provided with very good MSS. I well remember, as if it were now, having attentively read and diligently excerpted a Latin *Chronicle* of this monastery written on parchment. . . . Among the entries in this *Chronicle*, under the year 1313, it is recorded that in this monastery of St. Catherine there lived and died Friar

* City College, New York. The main results of this study were summarized in lectures delivered on 9 February 1954 to the Research Seminar of the National Institute of Optics at Arcetri, Italy; on 17 October 1955 to the Metropolitan New York Section of the History of Science Society; on 1 November 1955 to the University Seminar on the Renaissance at Columbia University; and on 30 December 1955 to the History of Science Society meeting jointly with the American Historical Association at Washington, D. C.

[1] *Miscellanea berolinensia ad incrementum scientarum*, 1710, *1*, 91.

[2] *Rivista di oftalmologia*, 1946, *1*, 140.

[3] An earlier effort to find out about the invention had been made by Manzini, Carlo Antonio. *L'occhiale all' occhio*. Bologna, 1660, p. 1. But he candidly confessed that despite a diligent search he could obtain no information.

[4] Not "Crucca," as in *Z. ophthal. Opt.*, 1914-15, 2, 92. Dati became a member on 29 November 1640. Andreini, Guido. *La vita e l'opera di Carlo Roberto Dati*. Florence, 1913; reprinted Milan, 1936, p. 26. Cited hereafter as "Andreini."

[5] Targioni Tozzetti, Giovanni. *Notizie degli aggrandimenti delle scienze fisiche accaduti in Toscana nel corso di anni LX del secolo XVII*. Florence, 1780, vol. 2 (cited hereafter as "Targioni"), p. 60. Republished under the title, *Atti e memorie inedite dell' Accademia del Cimento*. That these were essentially two issues of the same work, differing only in title, escaped Petella, Giovanni. Sull' invenzione degli occhiali. *Ann. Med. nav. colon.*, 1901, *7:1*, 825. Petella also misdated the publication in 1760. Chi l'inventore degli occhiali? *Ann. Med. nav. Colon.*, 1922, *28:2*, 62. Andreini (p. 12) misquoted the key word of the earlier title as "apprendimenti."

Alessandro[6] Spina,[7] a monk of most excellent character and most acute mind, who understood everything that he heard said or saw done; and when it happened that somebody else was the first to invent eyeglasses[8] and was unwilling to communicate the invention to others, all by himself he made them and goodnaturedly shared them with everybody.

This is the substance of what is said by the *Chronicle,* in my opinion a most authoritative and trustworthy source.

Dati's *Veglia* utilized what is still our most important piece of information about the invention of eyeglasses, the passage concerning Spina in the *Ancient chronicle of the Dominican monastery of St. Catherine in Pisa.* But his description of Spina's intelligence contains a highly significant expression; for according to Dati, Spina "understood everything that he heard said or saw done" (apprendendo tutto quello che udiva dire, o vedeva fare). Now the *Chronicle* still survives,[9] and what it actually says about Spina is in one respect strikingly different from Dati's version. For the *Chronicle* relates that "whatever had been made, when he [Spina] saw it with his own eyes, he too knew how to make it" (quae vidit oculis facta, scivit et facere).[10] Whereas Dati spoke of Spina's

[6] Not "Guglielmo," as in *Riv. Stor. crit. Sci. med. nat.,* 1921, *12,* 32.

[7] The material from this point to the end of the paragraph was enclosed within quotation marks by R. Greeff, who translated the *Veglia* into German. Die Veglia des Carlo Dati über die Erfindung der Brillen. *Z. ophthal. Opt.,* 1917, *5,* 75. Yet Greeff knew that the *Veglia* here differed from the *Chronicle* (*Z. ophthal. Opt.,* 5, 66; *6,* 8; *Die Erfindung der Augengläser,* Berlin, 1921, p. 78). Failing to realize that Dati was only paraphrasing, not quoting, the *Chronicle,* Greeff accounted for the differences by a false explanation, which we shall examine soon. His translation of the present passage is marred by several serious misunderstandings, repeated in *Erfindung,* p. 77. Many other passages of the *Veglia* fare no better at his hands.

[8] The speaker does not cite the *Chronicle* as saying that Spina "either was the inventor or did what someone else had done," despite Andreini's inaccurate summary (p. 151).

[9] Vitelli, Camillo. Index codicum latinorum qui Pisis in bibliothecis conventus S. Catherinae et universitatis adservantur. *Studi italiani di filologia classica,* 1900, *8,* p. 372, no. 78: Chronica conventus antiqua S. Catherinae Pis.
Mazzatinti, Giuseppe and Sorbelli, Albano. *Inventari dei manoscritti delle biblioteche d'Italia.* Florence, 1917, vol. 24, p. 79, no. 72 (78), ed. Gino Tamburini.
Singer, Charles. Steps leading to the invention of the first optical apparatus (*Studies in the history and method of science.* Oxford, 1917-21, vol. 2, p. 399) and Watson, Earnest Charles: The invention of spectacles (*Amer. J. Phys.,* 1953, *21,* 556) mistakenly described the *Chronicle* as "lost." For an opportunity to examine it as well as other documents at the Biblioteca Cateriniana of the Archiepiscopal Seminary of Pisa, I wish most cordially to thank Father Pierluigi Toniolo, director of that library. Dr. Toniolo went far out of his way to assist me at other Pisan institutions, and also put books in his personal library at my disposal.

[10] The *Chronicle* was published by Bonaini, Francesco. *Archivio storico italiano,* 1845, *6:2,* 397-593. The quoted passage occurs at p. 476, with a facsimile at pp. 396-397, no. III. Hereafter the *Chronicle* will be cited by reference to Bonaini's edition, which appeared in 1845, not 1848 (as in *Arch. stor. ital.,* 1920, *78:1,* 5; *Arte e storia,* 1920, *39,* 120; *Il giornale d'Italia,* 8 September 1920, p. 3; *Riv. Stor. crit. Sci. med. nat.,* 1921, *12,* 29; *Enciclopedia italiana,* vol. 25, p. 115). Pio Tommaso Masetti, *Monumenta et antiquitates veteris disciplinae ordinis praedicatorum ab anno 1216 ad 1348,* Rome, 1864, vol. 1, p. 340, dated the publication of the *Chronicle* in 1848, but in 1850 at vol. 1, p. 22. Stefano Orlandi, *"Necrologio" di S. Maria Novella,* Florence, 1955, vol. 2, p. 78, at first deprived the *Chronicle* of any date of publication and then assigned it to 1848 at vol. 2, p. 618, where he also misnumbered the volume as 4 instead of 6.

understanding everything that he *heard*, the *Chronicle* stresses Spina's *knowing how to make* what he *saw*. The difference between these two statements is of utmost importance to us. For as Dati told the story, Spina need not have seen any eyeglasses made by the anonymous inventor. All that Spina would require, according to Dati, was to hear about eyeglasses (since he understood everything that he heard) before proceeding to make them by himself (egli da per se stesso gli fabbricò). But the *Chronicle*, by saying "whatever had been made, when he saw it with his own eyes, he too knew how to make it," plainly implies that Spina did see eyeglasses made by the inventor before constructing his own.

Why did Dati's *Veglia* misrepresent the *Chronicle*? Dati was a professor at the University of Florence, and although he visited Pisa at least once,[11] there is nothing to indicate that he ever personally examined the *Chronicle*.[12] He was supplied with excerpts from Pisan MSS by his friend Francesco Redi (1626-97), chief physician to the Grand Duke of Tuscany.[13] From Pisa on 26 February 1674 Redi sent to Dati at Florence a letter containing the passage about Spina in the *Chronicle*.[14] But instead of the actual text (quae vidit oculis facta, scivit et facere; whatever had been made, when he saw it with his own eyes, he too knew how to make it) Redi transmitted the following reading: quaecumque vidit aut audivit facta scivit et facere[15] (whatever he saw or heard had been made, he too knew how to make it). That Redi wrote "quaecum-

11 Toward the end of January 1655. *Lettere inedite di alcuni illustri accademici della Crusca*. Pesaro, 1835, p. 46; 2d ed., Florence, 1837, p. 63. These two editions were made available to me by the Chancellor of the Accademia della Crusca, Dr. Francesco Pagliai, whom I wish to thank most heartily for his friendly co-operation.

12 The *Chronicle* was (and still is) in Pisa, not Florence, despite Gordon, Benjamin L. A short history of spectacles. *J. med. Soc. N. J.* 1951, *48*, 5.

13 Redi attended the University of Pisa, where he never was a "professor of medicine," a post to which he was erroneously assigned by Hirschberg, Julius. *Geschichte der Augenheilkunde* (vol. 12-15 of the *Handbuch der gesamten Augenheilkunde*, 2d ed.). Leipzig, Berlin, 1899-1918, vol. 13, p. 270. Hirschberg's blunder was repeated by Lundsgaard, K. K. K. Brillernes historie. *Med.-hist. Smaaskr.* 1913, *6*, 15; by Greeff. Die Briefe des Francesco Redi über die Erfindung der Brillen. *Z. ophthal. Opt.*, 1918, *6*, 1. *Erfindung*, p. 66; and by Sorsby, Arnold. *A short history of ophthalmology*. London, 1933; 2d ed., 1948; p. 70. For a list of the professors of medicine at Pisa between 1622 and 1737, see Fabroni, Angelo. *Historia academiae pisanae*. Pisa, 1791-95, vol. 3, pp. 688-689; see vol. 2, p. 64, and vol. 3, p. 225, for Redi as a student at Pisa. Hirschberg dated Redi's death in 1694, a mistake found also in Singer, *Studies in the history . . . of science*, vol. 2, p. 399, and in *Enciclopedia italiana*, vol. 10, p. 250. Redi's death was put in 1670 by Golesceano, Constantin. Contribution à l'histoire des lunettes. *Clin. prat. méd.-chir.*, 1914, *10*, 28, and in 1688 by De Waard, Cornelis. *Die uitvinding der verrekijkers*, The Hague, 1906, p. 63.

14 Since we must deal with two earlier letters written by Redi to Dati about eyeglasses, this letter of 26 February 1674 will hereafter be designated "Redi-Dati No. 3." Despite Targioni, vol. 1, p. 70, Redi did not write a letter about eyeglasses in 1678 to Dati, who died in 1676.

15 *Opere di Francesco Redi* (*Classici italiani*, vol. 169-177). Milan, 1809-11, vol. 7, p. 254.

que" instead of "quae" need not detain us now. But his substitution of "aut audivit" for "oculis" is another matter. Although the *Chronicle* is admittedly difficult to decipher, Redi could not have honestly but mistakenly read "aut audivit" in place of "oculis." There can be absolutely no doubt that he deliberately suppressed "oculis" and with malice aforethought introduced "aut audivit." Although rightly honored as the founder of experimental entomology, as a poet of considerable talent, and as a scholar of prodigious erudition, he seems also to have taken a perverse pleasure in perpetrating literary frauds.[16] His intention in this instance was to deceive not Dati alone but, through Dati, the world at large.

[16] Volpi, Guglielmo. Sandro di Pipozzo. *Rivista delle biblioteche e degli archivi*, 1909, *20*, 65-72. Francesco Redi e un antico trattatello della cura delle malattie. *Arch. stor. ital.*, 1915, *73:1*, 101-110. Le falsificazioni di Francesco Redi nel Vocabolario della Crusca. *Atti della r. Accademia della Crusca*, 1915-16, pp. 33-136.

Volpi's baptismal name was not "Alessandro," as he was christened by Albertotti, Giuseppe. Note sugli occhiali. *Atti Accad. Padova*, 1913-14, *30*, 311. Note critiche e bibliografiche riguardanti la storia degli occhiali. *Ann. Ottalm.*, 1914, *43*, 336 (cited hereafter as "Albertotti, 1914"). "Alessandro" was repeated by the editor of *Riv. Stor. crit. Sci. med. nat.*, 1914, *5*, 321, while the full name of the distinguished lexicographer and historian of literature was altered to "Alessandro Vulpi" by Greeff, *Z. ophthal. Opt.*, 1914-15, *2*, 93.

The first of these three articles by Volpi has as its title the name coined by Redi for an author who never existed save in his own mind. He professed to own a MS composed by this figment and containing the earliest reference to eyeglasses. He took good care, however, never to submit this MS to the scrutiny of scholars by publishing it, despite the assertion to the contrary by Barck, Carl. The history of spectacles. *St. Louis med. Rev.*, 1904, *49*, 306. *Open court.*, 1907, *21*, 214; followed by Singer, *Studies in the history . . . of science*, vol. 2, p. 399; by Andrew, Llewelyn. Some notes on the history of spectacles. *Transactions of the Lancashire and Cheshire antiquarian society*, 1925, *42*, 4; and by Watson, *Amer. J. Phys.*, 1953, *21*, 555-556. Barck assigned the supposed publication of the MS to 1648, when Redi was twenty-two. Actually his first publication came from the press sixteen years later, in 1664; and he cited (not published) the imaginary MS in 1678 (not 1648, despite Bock, Emil. *Die Brille und ihre Geschichte*. Vienna, 1903, p. 12). Redi pretended that it had been written in 1299, not 1292, as in Rouyer, Joseph. *Coup d'oeil rétrospectif sur la lunetterie*. 2d ed., Paris, 1901, p. 19, who was followed by Mme. Alfred Heymann in her sumptuously illustrated but somewhat unreliable *Lunettes et lorgnettes de jadis*. Paris, 1911, p. 4; nor 1289, as in Barck, who was followed by Singer, *Studies in the history . . . of science*, vol. 2, p. 399; by Garrison, Fielding H. *An introduction to the history of medicine*, 3d ed., Philadelphia, London, 1921, p. 178; by Cole, Rufus. Francesco Redi. *Ann. med. Hist.*, 1926, *8*, 349; by Utstein, Gertrude. Spectacles in the making. *Sight Sav. Rev.*, 1931, *1:2*, 42; and by Sarton, George. *Introduction to the history of science*. Baltimore: Williams & Wilkins, 1927-48, vol. 2, p. 1025; nor 1285, as in Andrew, *op. cit.*, p. 4, and Gordon, *J. med. Soc. N. J.*, 1951, *48*, 6. The date (1299), feigned by Redi for his non-existent MS, was transferred to the *Chronicle*, which was still being written as late as 1411 (*Chronicle*, p. 582), by Rouyer, p. 19; Mme. Heymann, *Lunettes*, p. 5; Reis, Wiktor. Ein literarisches Brillendokument aus dem Anfang des XIV jahrhunderts. *Folia ophthal. orient.*, 1932-34, *1*, 399; and Greeff (*Z. ophthal. Opt.*, 1914-15, *2*, 93), according to whom the *Chronicle* was composed by the author dreamed up by Redi. He called his "MS" a treatise on the management of the family; not a letter, despite Heller, August. *Geschichte der Physik*. Stuttgart, 1882-84, vol. 1, p. 201, and Landolt, Marc. Qui fut l'inventeur des lunettes? *Hygiène, Paris*, 1912, *3*, August, p. 12; nor a chronicle, despite Hirschberg, *Mitt. Gesch. Med. Naturw.*, 1907, *6*, 221, and the editor of *Riv. Stor. crit. Sci. med. nat.*, 1914, *5*, 321; nor a governmental treaty, despite Greeff, *Z. ophthal. Opt.*, 1918, *6*, 10, and *Erfindung*, p. 83; nor a contract, despite Rohr, Moritz von. *Naturwissenschaften*, 1919, *7*, 210. As an outstanding scientist of the 17th century, Redi did not write the MS in 1299, despite Heller, *loc. cit.* Nor was it published by Robert Smith in 1755, despite Hoppe, Edmund. *Geschichte der Physik*. Braunschweig, 1926, p. 250. Nor was it a chronicle of the monastery in which Pisa was a monk, despite Greeff, *Z. ophthal. Opt.*, 1914-15, *2*, 93.

I submit, then, that Dati's divergence from the *Chronicle* was due to his acceptance of Redi's falsification. This explanation conflicts with an idea put forward by Giuseppe Albertotti (1851-1936), professor of ophthalmology at the University of Padua and director of the eye clinic in that city, who maintained that Dati quoted the *Chronicle* from memory before hearing from Redi.[17] Albertotti's thesis was repeated with an embellishment by Richard Greeff (1862-1938), professor of ophthalmology at the University of Berlin and director of the Charité eye clinic. Greeff held that "Dati cited, obviously from memory, the interesting passage about the invention of eyeglasses which he had found in a *Chronicle* of the monastery of St. Catherine in Pisa"; and that "Dati, however, had already cited this passage incorrectly from memory. Had Redi's letter been before him, hence the precise text, he would surely have quoted this and not from memory."[18] Greeff here referred to the letter sent by Redi to Dati on 8 November 1673[19] (which we shall call "Redi-Dati No. 2"). In this letter Redi copied out for Dati "the precise text" of a passage, not in the *Chronicle*, but in another Pisan MS, as Albertotti pointed out. The *Chronicle* passage was transmitted, not in Redi-Dati No. 2, but in No. 3. It was the falsification contained in No. 3 that led Dati astray, not his faulty memory.

Albertotti and Greeff, who gratuitously assumed that Dati saw (and even found) the *Chronicle* passage before receiving No. 2, adduced no evidence to show that he saw it at any time. But even if he had examined it before hearing from Redi, he was no falsifier of texts,[20] like his mischievously ingenious friend. And even if he were, how likely is it that he would have chosen to distort exactly the same passage in exactly the same way? Is it not much more probable that Dati never laid eyes on the *Chronicle* and trustingly adopted the version sent to him by Redi, whose scholarly dishonesties were so cleverly concealed that they were first suspected some two centuries after his death? In dedicating the *Veglia* to Redi, Dati said it became valuable when Redi communicated to

17 Albertotti, 1914, p. 330. His idea was accepted by Cirincione, Giuseppe. L'invenzione degli occhiali è gloria italiana. *Ann. Ottalm.*, 1927, *55*, 932.

18 *Z. ophthal. Opt.*, *5*, 66; *6*, 2; *Erfindung*, pp. 78, 80. The Albertotti-Greeff thesis was accepted by Von Rohr, *Naturwissenschaften*, 1919, *7*, 209.

19 Not "8 November 1663," as in Albertotti, 1914, pp. 330, 335, 354, and in Greeff, *Erfindung*, p. 82.

20 Yet see n. 36, below.

him the "recondite notice that closes it" (Targioni, p. 50). Was he not referring to the Spina passage in the *Chronicle?*[21]

Redi was thoroughly familiar with the working habits of his friend Dati. The latter would today be called a perfectionist; never satisfied with what he wrote, he was constantly revising, cancelling, adding. In fact, when he died on 11 January 1676, he left the *Veglia* unpublished and even unfinished. Hence, although he read it at a meeting of Crusca late in May, 1673, Redi knew perfectly well that the *Chronicle* excerpt, which he sent in falsified form to Dati on 26 February 1674 (Redi-Dati No. 3), could still be depended on to affect the *Veglia's* account of Spina's acquaintance or nonacquaintance with the work of the anonymous inventor.

The Pisan Annals

The date of the Crusca meeting at which Dati read the *Veglia* may be inferred from the following letter that he received from Redi:[22]

> Last night Lorenzo Panciatichi[23] told me that at our[24] Accademia della Crusca you read one of your learned *Veglie,* in which you virtually demonstrated that the invention of eyeglasses is not ancient but modern, and a little before 1300. I congratulate you, and regret not having had the good fortune to be present. I hope however that I can make up this loss by going to your house some morning to beg you to let me hear it.

When the above letter was printed, it bore the date 31 May 1663, which has been shown to be ten years too early and should be changed to 31 May 1673.[25]

21 Disregarding everything but the word "closes," Petella (*Ann. Med. nav. colon.,* 1901, *7:1,* 826) identified Redi's communication with the *Veglia's* final paragraph. But this, far from being a "recondite notice," is instead a conclusion which Dati found in a printed book (cf. n. 261, below), probably long before Redi contributed anything to the *Veglia.*

22 Fontani, Francesco. *Elogio di Carlo Roberto Dati.* Florence, 1794, p. 187; reprinted by Albertotti, 1914, pp. 330-331.

23 Lorenzo Panciatichi (1635-76) was admitted to membership in the Accademia della Crusca on 12 August 1654 (*Lettere di Francesco Redi,* ed. Domenico Moreni, Florence, 1825, p. 179). Albertotti's misspelling (1914, p. 330) of his surname was sedulously followed by Greeff, *Z. ophthal. Opt.,* 1917, *5,* 66, although the family was included in Litta, Pompeo. *Famiglie celebri italiane.* Milan, Turin, Naples, 1819-1923. For a brief account of Lorenzo's life, see Passerini, Luigi. *Genealogia e storia della famiglia Panciatichi.* Florence, 1858, pp. 223-230; or Litta, vol. 9, Panciatichi of Pistoia, table 14, with a bust of Lorenzo on the last page of the volume. His writings were collected and published by Cesare Guasti (Florence, 1856).

24 Redi joined the Accademia della Crusca in 1655; on 23 December, according to his own recollection (*Opere,* vol. 8, pp. 112-113); but on 13 November, according to the organization's list (*Lettere di Redi,* ed. Moreni, p. 183; on p. 184, "1665" is a misprint).

25 Rosen, Edward. Carlo Dati on the invention of eyeglasses. *Isis,* 1953, *44,* 4-5.

From this letter (hereafter designated "Redi-Dati No. 1") it seems clear that Redi's interest in the invention of eyeglasses was first aroused by the *Veglia* in May, 1673,[26] when Dati still knew nothing about the Spina passage in the Pisan MSS. Nearly half a year later, on 8 November 1673, in Redi-Dati No. 2, Redi said:

> With reference to the inventor of eyeglasses, I shall set down here below the exact words of the manuscript chronicle of the monastery of St. Catherine at Pisa:
>
> Friar Alessandro Spina of Pisa made with his own hands whatever he wished, and shared it with others, since he was dominated by kindliness. At that time, through a beautiful, useful and novel invention, somebody invented the glass lenses which are commonly called "eyeglasses" and did not want to communicate to anybody the art of making them.[27] Having seen them, this good man, a craftsman, mastered them immediately without anybody teaching him, and he taught others who wanted to know. He sang melodiously, wrote elegantly and decorated the handwritten books which are called "illuminated manuscripts." In short, there was none of the manual arts that he did not know.[28]

Redi went on to say: "This chronicle is written in a book of small folio size and of thick ordinary paper." The last detail shows that Redi was not yet well acquainted with the Pisan MSS. For the *Chronicle* was not written on ordinary paper. The excerpt about Spina in Redi-Dati No. 2, although labeled "the exact words of the manuscript chronicle," occurs not in the *Chronicle* but in a companion MS, the *Annals of the Dominican monastery of St. Cath-*

[26] Even so painstaking a student as Andreini accepted (p. 151) the erroneous date of 30 May 1663 for the *Veglia*.

[27] "Nobody wanted to communicate the art of making them to him [Spina]" was how this clause was mistranslated by Gerland, Ernst. *Geschichte der Physik*. Munich, Berlin, 1913, p. 203.

[28] *Opere*, vol. 5, pp. 82-83; cf. *Chronicle*, p. 477, n. 134 (in line 5, read "ipsam"). Domenico Maria Manni, *Degli occhiali da naso inventati da Salvino Armati gentiluomo fiorentino trattato istorico* (cited hereafter as "Manni"), pp. 53-54, printed Redi-Dati No. 2, and from it concluded that Redi was the first to discuss eyeglasses. But the priority belongs to Dati, as is clear from Redi-Dati No. 1 (the beginning of which was quoted just above). It was first published, however, six years after Manni's death in 1788 (cf. n. 22, above). He lived to be almost a hundred years old, having been born in 1690, not 1670, the date given by Masson, Albert. Notes sur l'histoire des lunettes. *France méd.*, 1908, 55, 202. There were two editions of Manni, both issued at Florence, the first in 1738, and the second in 1741. There was no edition in 1638, despite Andreini, p. 152; nor in 1758, despite Targioni, vol. 1, p. 70, followed by Rocchi, Vincenzo. Appunti di storia critica del microscopio. *Riv. Stor. crit. Sci. med. nat.*, 1913, *4*, 4; nor in 1789, despite Barduzzi, Domenico. Salvino degli Armati non fu l'inventore degli occhiali. *Riv. Stor. crit. Sci. med. nat.*, 1921, *12*, 31. Prior to the editions of 1738 and 1741, Manni had published an earlier version of the work in the form of two *Dell' invenzione degli occhiali da naso ragionamenti accademici* in Angelo Calogerà's *Raccolta d'opuscoli scientifici, e filologici*, vol. 4 (Venice, 1730), pp. 37-131. These *Ragionamenti* (as we shall designate them hereafter) were reprinted in *Scelta di dissertazioni cavate da' più celebri autori*, Venice, 1750-51, tom. 2, pt. 1, pp. 113-218, and, together with the edition of 1738, were condensed and translated into German. *Allgemeines Magazin der Natur, Kunst und Wissenschaften*, 1756, *7*, 1-25.

erine in Pisa.[29] These *Annals* were written on paper[30] 21 x 27 cm. (8 x 11 in.) and might be classified as a small folio. They were compiled in the mid-sixteenth century[31] by an anonymous friar who amplified what he found in the *Chronicle,* which had been written in the fourteenth and early fifteenth centuries by a series of four friars whose identities are known.[32]

Why did Redi falsify the Chronicle?

Redi candidly admitted that "one cannot notice everything even by exercising the utmost care."[33] Some four months after he had confused the *Annals* with the *Chronicle,* he knew these MSS better.[34] In fact, he knew them well enough to transmit a falsified excerpt from the *Chronicle,* as we saw above, in Redi-Dati No. 3, which began as follows (*Opere,* vol. 7, pp. 252-253):

[29] Vitelli, *Studi italiani di filologia classica,* 1900, *8,* p. 348, no. 42: Annales conventus sanctae Catharinae de Pisis ordinis praedicatorum. Mazzatinti and Sorbelli, *Inventari,* vol. 24, p. 75, no. 40 (42). Redi's blunder (attributing to the *Chronicle* the passage about Spina in the *Annals*) unfortunately was repeated by the foremost student of the Italian middle ages, Lodovico Antonio Muratori (1672-1750), in a work published originally in Latin, then in Italian, and reissued over half a dozen times in both languages. *Antiquitates italicae medii aevi.* Milan, 1738-42, vol. 2, col. 396. *Dissertazioni sopra le antichità italiane.* Milan, 1751, vol. 1, p. 369. Muratori's example was followed by scholars too numerous to list. He discussed the invention of eyeglasses in Dissertation 24 (The arts in Italy after the fall of the Roman Empire), not Dissertation 30 (Markets and trade in the Dark Ages), despite Cancellieri, Francesco. *Lettera . . . al ch. sig. dottore Koreff . . . sopra il tarantismo.* Rome, 1817, p. 292; nor Dissertation 51 (The origin and development of the Guelf and Ghibelline factions in Italy), despite Del Lungo, Isidoro. Le vicende d' un' impostura erudita. *Archivio storico italiano,* 1920, *78:1,* 35-36 (cited hereafter as "Del Lungo").

[30] Not parchment, as in Vitelli's inaccurate description, repeated by Tamburini (in Mazzatinti and Sorbelli); cf. Albertotti, 1914, p. 354, no. 4.

[31] Not "toward the end of the sixteenth century," as was said by Canovai, Stanislao. Alessandro della Spina Domenicano. *Memorie istoriche di più uomini illustri pisani* (cited hereafter as "*Memorie*"), Pisa, 1790-92, vol. 2, p. 246, followed by Del Guerra, Giorgio. Alessandro della Spina inventore o primo preparatore di occhiali. *Ann. Ottalm.* 1933, *61,* 383. *Riv. Stor. crit. Sci. med. nat.,* 1933, *24,* Atti of the 4th National Congress, p. 17. Cf. *Chronicle,* pp. 604, 627.

[32] The *Chronicle* was begun by Bartolomeo da San Concordio (†1347) and was continued by Ugolino di ser Novi dei Cavallosari (†1364), by Domenico da Peccioli (†1408), and by Simone da Cascina in 1411; cf. *Chronicle,* pp. 400-401, 524, 582, 589. Simone da Cascina's handwriting begins at fol. 39*r* in the MS, the preceding sheets (fol. 1*r*-38 *v*) being all in the handwriting of Domenico da Peccioli, who must have recopied what had been composed by both his predecessors. For the date of Ugolino's death, see Orlandi, *Necrologio,* vol. 2, p. 535.

[33] *Opere,* vol. 5, p. 84; letter to Dati, from Pisa, 5 March 1674.

[34] The same cannot be said for Manni, p. 54. He accepted the (erroneous) ascription to the *Chronicle* of the Spina passage quoted in Redi-Dati No. 2. Unaware that it was taken from the *Annals,* he sought to explain its divergences from the *Chronicle* passage about Spina by gratuitously supposing that there were two different treatments of Spina in the *Chronicle.* The only one in it was "somewhat altered" and extended by Redi, thought Ranieri Tempesti (*Discorso accademico sull' istoria letteraria pisana,* published anonymously at Pisa in 1787, p. 59), who also failed to realize that Redi was quoting from the *Annals,* not from the *Chronicle.* With reference to this topic, Greeff confessed: "Formerly I myself was always puzzled and sought in vain for an explanation." But his whole treatment of the subject (*Z. ophthal. Opt.,* 1918, *6,* 8-9; *Erfindung,* pp. 80-82) shows that finally as well as formerly he was inextricably entangled in a mass of confusions.

Here is a long letter to you in which I shall write you what I recently discovered concerning what I told you by word of mouth about the inventor of eyeglasses. . . . I shall be pleased if the whole thing can serve to enrich your learned *Veglia*. . . . I tell you then that in the library of the Dominican monastery of St. Catherine in Pisa there is an old book in parchment, large quarto, which starts as follows: "Here commences the *Ancient chronicle of the Dominican monastery of St. Catherine in Pisa.*"

By contrast with Redi-Dati No. 2, in No. 3 Redi was aware that the *Chronicle* was written on parchment, not paper, and he put it in the category of large quarto, not small folio.[35] It was in No. 3, while professing to transcribe the Spina passage from the *Chronicle*, that he substituted "aut audivit" for "oculis."

What was the motive behind this falsification of the *Chronicle*? Why was it desirable from Redi's standpoint that Spina should be depicted as capable of making eyeglasses without having previously seen the work of the original inventor? Was the purpose simply to exaggerate Spina's abilities?

Redi's Accomplices

Before attempting to answer these questions, we should note that Dati was a party to the conspiracy. True, he did not go as far as Redi and alter the reading of a text. But he excluded from the *Veglia* a crucial statement in the *Annals*.[36] For the excerpt from that MS (mislabeled the *Chronicle*) which he received in Redi-Dati No. 2 contained the following passage, which was quoted above, but which we shall now find it useful to look at again (*Opere*, vol. 5, p. 83; Manni, p. 53):

At that time, through a beautiful, useful and novel invention, somebody invented the glass lenses which are commonly called "eyeglasses" and did not want to communicate to anybody the art of making them. Having seen them, this good man [Spina], a craftsman, mastered them immediately without anybody teaching him . . .

By saying "Having seen them" (illis visis),[37] the annalist made explicit what is implied in the *Chronicle*, namely, that Spina saw

35 The *Chronicle* measures 15 x 22 cm., a good deal smaller than the *Annals'* 21 x 27 cm.

36 While publishing some letters by Galileo's foremost disciple, Evangelista Torricelli, Dati similarly suppressed an inconvenient sentence (Andreini, p. 142). If Andreini was right in describing (p. 182) Dati's prose as "always . . . accurate," he displayed due caution in saying (p. 12) that Dati "in his writings shows himself to be sincere most of the time."

37 These words were stigmatized as "most uncertain" by Del Guerra. *Ann. Ottalm.* 1933, *61*, 384. *Riv. Stor. crit. Sci. med. nat.* 1933, *24*, Atti, p. 18. But he failed to explain why they are less certain than any other portion of the *Annals*.

spectacles made by the original inventor before constructing any of his own.[38]

The relation between these two documents was seen in an entirely different light by Stanislao Canovai. An ardent admirer of Alessandro Spina, Canovai said (*Memorie,* vol. 2, p. 246):

> It seems that he [the annalist] could not imagine how Alessandro could make eyeglasses without first seeing those already made, and therefore he added "illis visis" on his own account against the clear statement of the *Chronicle,* which assures us that the inventor of eyeglasses was unwilling to communicate not merely the art of making them but even the very eyeglasses he had already made.

It is not the *Chronicle* but the *Annals* which assures us that the inventor of eyeglasses "did not want to communicate to anybody the art of making them." Why did Canovai attribute this statement to the wrong MS? Was he perhaps misled by Redi-Dati No. 2?

Canovai corrected (*Memorie,* vol. 2, p. 245, n. 10) Redi's confusion of the *Chronicle* with the *Annals.* From the *Chronicle* passage about Spina, Canovai quoted (*op. cit.,* p. 243, nn. 3, 5) a whole sentence omitted by Redi (*Opere,* vol. 2, p. 260; vol. 7, p. 254). He also put the true reading "quae" in place of Redi's faulty "quaecumque"; and altered another of Redi's readings which we shall have occasion to discuss later on. There can be no question, then, that Canovai personally examined the *Chronicle.*

He must have known, therefore, that it does not contain the statement (which he deliberately attributed to it) that the inventor of eyeglasses "did not want to communicate to anybody the art of making them." Furthermore, he must have recognized Redi's fraudulent substitution of "aut audivit" for "oculis." Instead of challenging this flagrant falsification, Canovai repeated it. In fact, he went far beyond Redi by imputing to Spina the ability "to understand the secret structure of machines and the unknown intentions of the inventor at a single glance, even by a single report" (*Memorie,* vol. 2, p. 237). But whereas Dati and Redi discreetly chose to keep the *Annals'* "illis visis" out of sight, Canovai brought that highly embarrassing expression out into the open. At the same time he vigorously impugned the reliability of the annalist, to whom, he charged (*op. cit.,* p. 246), "it frequently happened, because of poor judgment or poor sense, to alter the truth" of the *Chronicle.*

[38] Neither MS attributed the invention to Spina, despite Barduzzi (*Riv. Stor. crit. Sci. med. nat.,* 1921, *12,* 31).

This low evaluation of the annalist's intelligence will hardly commend itself to persons free from Canovai's strong bias in favor of Spina. For is not "illis visis" simply the annalist's application of the *Chronicle's* general observation about Spina ("whatever had been made, when he saw it with his own eyes, he too knew how to make it") to the specific case of eyeglasses?[39] The *Annals'* "illis visis" stands in the way of Redi's "aut audivit." The former had to be undermined if the latter was to triumph. Here, I submit, is the concealed motive behind Canovai's unjustified condemnation of the annalist. His onslaught, together with his transfer to the *Chronicle* of the *Annals'* statement about the inventor's unwillingness to communicate the art, must be viewed as part of a conscious plan to help propagate Redi's fraud, "aut audivit."

Another Pisan writer, Tempesti, a few years before Canovai, had likewise aided in spreading Redi's "aut audivit." Unlike Canovai, Tempesti failed to recognize that the excerpt attributed by Redi to the *Chronicle* was actually the *Annals* passage concerning Spina (see n. 34, above). Nevertheless, like Canovai, he knew the *Chronicle* at first hand. For he too quoted (p. 59) the sentence ("Cantare . . . valent") omitted by Redi, and wrote "quae," not "quaecumque." He also replaced Redi's false reading "aliquo" by the correct reading "alio." Moreover, he inserted a word ("omnibus") inadvertently[40] dropped by Redi, and deleted a word ("et" between "facta" and "communicare") introduced by Redi without MS authority. Thus the evidence is conclusive that Tempesti was personally acquainted with the *Chronicle*. Yet he repeated Redi's substitution of "aut audivit" for "oculis."[41] Not only Canovai, therefore, but Tempesti also must be designated a co-conspirator with Redi. The latter had misread the *Chronicle's* opening word

39 Hence I cannot agree with Bonaini's judgment: "As Canovai very wisely noted, the author of the *Annals* added on his own account that Friar Alessandro saw the eyeglasses of the first inventor" (*Chronicle*, p. 478).

40 "Inadvertently," because it is represented by "tutti" in Redi's Italian paraphrase (*Opere*, vol. 2, p. 261), while "omnibus" appears in Redi-Dati No. 3 (*Opere*, vol. 7, p. 254) and is therefore reflected in Dati's *Veglia* (Targioni, p. 60). Moreover, "omnibus" occurs in the original version of Redi's *Lettera* (see n. 47, below). This earlier draft is still preserved in manuscript form at the National Central Library of Florence (hereafter designated "BNC") as Palatino 1208, Striscio 1370, p. 7.

41 Redi's mutilated version of the *Chronicle* passage about Spina was accepted by Del Guerra (*Ann. Ottalm.*, 1933, *61*, 383; *Riv. Stor. crit. Sci. med. nat.*, 1933, *24*, Atti, p. 17). He failed to notice the discrepancies between Redi's readings and the *Chronicle* passage, although he published a photocopy of the latter (fig. 2; *Ann. Ottalm.*, p. 382; Atti, p. 15). Coupling it with a facsimile of the *Annals* passage about Spina (fig. 3; *Ann. Ottalm.*, p. 383; Atti, p. 17), he claimed that both were "reproduced photographically for the first time." Yet (as was pointed out by Scalinci, Noè. A proposito di Alessandro della Spina e di storia della invenzione degli occhiali. *Riv. Stor. crit. Sci. med. nat.*, 1933, *24*, 140) Del Guerra cited Albertotti, 1914, who had published photocopies of both passages (p. 334, fig. 1: *Chronicle;* p. 335, fig. 2: *Annals*).

"Interroga" as "In toga."[42] When Tempesti (p. 37) corrected this error, he blamed it on a French translator of Redi (cf. n. 105, below), instead of on Redi himself.

Spina and Galileo

Having ascertained the alignments of these two Pisan authors, Tempesti and Canovai, let us now look more closely at the *Veglia.* From childhood on, Dati had known Galileo intimately and admired the great scientist immensely. Like many a contemporary Italian intellectual, the younger man courageously resolved to honor Galileo's memory, despite his condemnation by the Roman Catholic church.[43] Dati's *Veglia* in fact belongs to the literature about Galileo. It opens with the arrival in Florence of a foreigner who has come from abroad for the express purpose of visiting the renowned astronomer. Because of his blindness the old man can entertain his guest in person but briefly and therefore asks certain of his disciples to look after the stranger. This is the setting created by Dati for the conversation which constitutes the core of the *Veglia.*

He has one of his characters say about Galileo (Targioni, p. 51):

> As he himself relates most sincerely, first in the *Sidereal message,*[44] then in the *Saggiatore,* news arrived in Venice that to Count Maurice in Holland there had been presented a telescope with which distant objects were seen as perfectly as if they were quite near, *and nothing more. On this very simple report* Galileo reflected, and in a short time he figured the instrument out.

The expressions translated in italics (e niente più. Sopra questa semplicissima relazione) emphasize that although Galileo heard about the telescope, he did not see it, as was alleged by his detractors.[45]

Is this not a remarkable analogy to the story of Spina and the eyeglasses? Only a single circumstance prevents the parallel from being perfect: the *Chronicle* implies and the *Annals* explicitly assert that Spina saw the spectacles made by the original inventor.[46]

42 *Opere,* vol. 2, p. 258; vol. 7, p. 253. The *Chronicle* begins (p. 399) by justifying itself with the following quotation from the Old Testament: "Interroga generationem pristinam, et diligenter investiga patrum memoriam" (Job 8:8; "Ask the previous generation, and diligently seek the record of your predecessors," as the chronicler understood this verse).

43 That Dati's stand required courage was emphasized by the Dominican Father Vincenzo Marchese, *Scritti vari,* 2d and 3d ed., Florence, 1860, 1892, vol. 2 (*Opere,* vol. 4), p. 274.

44 Rosen, Edward. The title of Galileo's *Sidereus nuncius. Isis,* 1950, *41,* 287-289.

45 *Le opere di Galileo Galilei,* national edition. Florence, 1890-1909; reprinted, 1929-39; vol. 10, p. 255, no. 233.

46 This point was heavily emphasized by Manni in his eagerness to claim the invention for a fellow-Florentine. *De florentinis inventis.* Ferrara, 1731, p. 54.

Is this not the reason why Dati carefully omitted from the *Veglia* the annalist's words "illis visis" (Having seen them), thereby falsely attributing to Spina a Galileo-like capacity to make what he had merely heard about without actually seeing it?

This dishonest attitude may have been imparted to Dati in the course of the conversations mentioned at the beginning of Redi-Dati No. 3. When Dati died two years later leaving the *Veglia* still in MS, Redi published his own *Letter concerning the invention of eyeglasses.*[47] Inserting in this *Lettera* his fraudulent version of the *Chronicle* passage (wherein he replaced "oculis" by "aut audivit"), he inferred from it (*Opere,* vol. 2, p. 260):

> that if Friar Alessandro Spina was not the first inventor of eyeglasses, at least it was he who by himself without any instruction rediscovered the method of making them; and that at the very time when he lived, this most useful invention first came to light. Exactly the same thing happened, by a certain coincidence of fate, to our most famous Galileo Galilei. Having heard the report that a certain Fleming had invented the long spyglass, which is called by the Greek name "telescope,"[48] he worked out a similar one solely with the aid of the theory of refractions, without ever having seen [what the Fleming invented].

Giordano's Sermon

If we now feel confident that we have uncovered the true reason why Redi deleted "oculis" from the *Chronicle* and Dati disre-

[47] *Lettera intorno all' invenzione degli occhiali* (Florence, 1678); not 1648, as in Pansier, Pierre. *Histoire des lunettes.* Paris, 1901, p. 1, and in Bourgeois, Alexandre. Recherches historiques sur l'origine des lunettes. *Trav. Acad. nat. Reims, 1903, 113,* 153. *Les besicles de nos ancêtres.* Paris, 1923, p. 73, and in Ovio, Giuseppe. *Storia dell' oculistica.* Cuneo, 1950, pp. 227, 1267 (cf. n. 16, above); nor 1668, as in Rocchi, *Riv. Stor. crit. Sci. med. nat.,* 1913, *4,* 4, and in Albertotti, 1914, p. 330; nor 1676, as in Oliver, George H. The history of the invention and discovery of spectacles. *Brit. med. J.,* 1913, *2,* 1050, in Smith, E. Temple. The history of spectacles. *Med. J. Aust.,* 1928, *15:2,* 579, in Sorsby, *Short history of ophthalmology,* p. 70, in Thompson, Charles J. S. The origin and development of spectacles. *The Connoisseur,* 1934, *94,* 233, and in Gordon, *J. med. Soc. N. J.,* 1951, *48,* 6. Redi addressed this *Lettera* in 1678 to Paolo Falconieri, who did not die in 1676, despite Bock, *Die Brille und ihre Geschichte,* p. 12. Perhaps Bock confused Paolo with his better known brother Ottavio, the classical archaeologist, who is often said to have died in 1676, although the correct date is 21 November 1675. Burman, Pieter. *Sylloge epistolarum a viris illustribus scriptarum.* Leiden, 1724-27, vol. 5, p. 577. Paolo, outliving his brother by almost thirty years, died on 13 March 1704. *Notizie istoriche degli Arcadi morti,* ed. G. M. Crescimbeni. Rome, 1720-21, vol. 2, p. 9. He had become a member of the Accademia della Crusca on 3 August 1663 (*Lettere di Redi,* ed. Moreni, p. 152).
Guglielmo Libri (*Histoire des sciences mathématiques en Italie,* Paris, 1838-41, vol. 2, p. 75) complained that the Milan edition of Redi's *Opere* (see n. 15, above), although announcing (vol. 7, p. 257) its intention of including the *Lettera,* failed to do so. Yet the *Lettera* was in the *Opere,* vol. 2, pp. 255-265. Libri's groundless complaint was echoed by Rognetta, Francesco. Recherches sur les lunettes. *L'expérience,* 1842, *9,* 145, and *Ann. Oculist., Paris,* 1842, supplementary volume 2, 106; by Narducci (see n. 69, below); and by Petella (*Ann. Med. nav. colon.,* 1901, *7:1,* 824).

[48] Rosen, Edward. *The naming of the telescope.* New York: Schuman, 1947. The similarity of Galileo's accomplishment to Spina's must have been an afterthought, since it is missing from the earlier draft of Redi's *Lettera,* mentioned in n. 40, above.

garded "illis visis" in the *Annals,*[49] let us next take another look at the beginning of Redi-Dati No. 1:[50]

> Last night Lorenzo Panciatichi told me that at our Accademia della Crusca you read one of your learned *Veglie,* in which you virtually demonstrated that the invention of eyeglasses is not ancient but modern, and a little before 1300.

When Dati read the *Veglia* to Crusca in May, 1673, let us recall, he still knew nothing about the Spina passages in the Pisan *Chronicle* and *Annals.* On what, then, did he base his conclusion that eyeglasses were invented "a little before 1300"?

In the *Veglia* he assigned to the interlocutor, whose long speech about Spina was quoted at the beginning of this article, the following answer to the question "When were eyeglasses invented?" (Targioni, p. 59):

> Not many years before 1300, according to the assertion of Friar Giordano[51] da Rivalto,[52] a most eloquent preacher of the order of St. Dominic, who

[49] Despite Del Lungo, p. 13, Redi's *Lettera* made no mention of the *Annals.* He ignored it, according to Albertotti, 1914, p. 335, because "illis visis" lessened Spina's originality. But Albertotti proposed no reason why Redi should have cared one way or the other about Spina's originality.

[50] Fontani, *Elogio di C. R. Dati,* p. 187; Albertotti, 1914, p. 330.

[51] Not "Giacomo," as he was christened by Albertotti, *Atti Accad. Padova,* 1913-14, *30,* 312; on the previous page Albertotti had used the right name. Both "Giordano" and "Giacomo" appear on the same page (*Riv. Stor. crit. Sci. med. nat.,* 1914, *5,* 321); in the same journal (1921, *12,* 30) the preacher's baptismal name was altered to "Girolamo" by Barduzzi.

[52] Our earliest sources call him "Friar Giordano (of Pisa)." He had already been dead a couple of centuries when the appellation "Giordano da Rivalto" first emerged, a new coinage evidently based on an inference from *Chronicle* entries (pp. 482, 543) concerning two of his relatives. The *Chronicle* says that one of them was his nephew (nepos), a relationship later mistakenly altered by the *Annals* to "brother" (germanus; cf. *Chronicle,* p. 545). The *Annals'* "germanus" was misattributed to the *Chronicle* by Pietro Cardosi on p. 376 of his *Memorie sacre delle glorie di Pisa,* written in 1675 and now preserved at BNC as MS II III 170 (formerly Magliabechi MS. class xxxviii, codex 20). Both of Giordano's relatives who are mentioned in the *Chronicle* came from a locality near Pisa called "Rivalto." Making him a brother (rather than uncle) of one of them strengthened the supposition that he too was born at Rivalto, and the name "Giordano da Rivalto" became established in the literature. This supposititious name was subsequently reinterpreted when it was pointed out (*Memorie,* vol. 3, p. 90; vol. 4, p. 153) that a Pisan family of his day bore the surname "Da Rivalto." But no evidence links Giordano with this family and the inference from his relatives, although attractive, is less than conclusive. Like the *Annals* and Cardosi, the *Notizie intorno alla vita del b. Giordano da Rivalto* (published anonymously at Pisa, 1834), p. 7, transformed Giordano's nephew into a brother; and Lodovico Orlandini, a priest whose ancestors once lived in Rivalto, advanced the utterly gratuitous claim that Giordano belonged to the Orlandini family. *Genealogia del b. Giordano da Rivalto.* Pisa, 1900, p. 22. The most recent student of Giordano, abandoning the nomenclature so long in vogue, has reverted to the name "Giordano of Pisa," as the preacher was known to his contemporaries (cf. n. 94, below). Galletti, Alfredo. *L'eloquenza (dalle origini al XVI secolo). Storia dei generi letterari italiani.* Milan, 1938, p. 147. Rivalto was near Pisa, not Piacenza, as was mistakenly asserted by Müller, Curt. *Die Brille, ihre Geschichte und ihre Entwicklung.* Berlin, 1911, p. 7, and Feldhaus, Franz Maria. *Die Technik der Vorzeit, der geschichtlichen Zeit und der Naturvölker.* Leipzig, Berlin, 1914, col. 138.

flourished and preached in Santa Maria Novella a little after that time. In[53] one of his sermons,[54] cited in our *Vocabolario* [*degli accademici della Crusca*, the famous Italian dictionary] from a MS belonging to me, he says: "It is not yet twenty years since there was found the art of making eyeglasses which make for good vision, one of the best arts and most necessary that the world has."

When this speaker, Filippo Pandolfini,[55] quoted from one of his own MSS, did he act in good faith or was he another Redi? Did he actually own a MS of Giordano's sermons, and did he reproduce the passage from it without any alterations?

At the Mediceo-Laurentian Library in Florence a MS of Giordano's sermons contains, in the last five lines of the second column on the recto of folio 57,[56] the passage quoted in the *Veglia* by Pandolfini. He formerly owned this MS, which still carries his name in two places: on a little piece of paper firmly glued to the first folio, recto, bottom margin; and on the last folio, recto. His MS[57] later passed into the possession of the Pucci family.[58] After the death of Giuseppe Pucci (1782-1838)[59] the heirs sold the family MSS in 1840[60] to the historian of mathematics, Guglielmo Libri,[61]

53 Not "after," as was said by Oliver (*Brit. med. J.*, 1913, 2, 1050), followed by Smith (*Med. J. Aust.* 1928, *15:2*, 579).

54 Not "treatises," as was said by Amati, Giacinto. *Ricerche storico-critico-scientifiche sulle origini, scoperte, invenzioni e perfezionamenti.* Milan, 1828-30, vol. 4, p. 144.

55 Pandolfini (1575-1655) was a disciple of Galileo, who proposed him for membership in the Accademia dei Lincei, to which he was admitted in 1614. *Opere di Galileo*, vol. 12, p. 18.9-11, and vol. 19, p. 267. *Mem. Accad. Lincei*, classe di scienze morali, storiche e filologiche, series 6, 1938-39, 7, 484. Pandolfini belonged also to the Accademia della Crusca, which he joined in 1601. *Lettere di Redi*, ed. Moreni, p. 137 (12 December), p. 179 (28 November).

56 Not "54," as in Albertotti, 1914, p. 356. The folio number refers to the modern, not the original, numeration.

57 The library of the Pandolfini family, when catalogued in the early 1500's (see *Catalogo della libreria Pandolfini*, Florence, 1884, p. 6), did not contain Giordano's sermons, which evidently were acquired later in the sixteenth century or early in the seventeenth. For the name of an owner earlier than Filippo Pandolfini, see Levasti, Arrigo. *Mistici del duecento e del trecento.* Milan, Rome, 1935, p. 1000. In 1691, a generation after Filippo Pandolfini's death, his MS belonged to Senator Ruberto Pandolfini, according to the 3d ed. of the *Vocabolario degli accademici della Crusca*, vol. 1, p. 43.

58 The unpublished *Catalogo dei manoscritti della libreria Pucci* listed, besides Pandolfini's, a second MS of Giordano's sermons, which it described as lacking the last of 258 leaves. Narducci, Enrico. *Prediche inedite del b. Giordano da Rivalto (Collezione di opere inedite o rare*, vol. 16). Bologna, 1867, p. xliii. This work will be cited hereafter as "Narducci, *Prediche inedite.*"

59 Migliarini, Arcangelo M. *Alcuni fiori alla memoria del marchese cavaliere Giuseppe Pucci.* Florence, 1839.

60 Not 1843, despite Léopold Delisle, *Notices et extraits des manuscrits de la Bibliothèque nationale*, 1886, *32:1*, 18. Perhaps he confused the year of their purchase in Florence (1840) with the year of their arrival in France (1843); see *Réponse de M. Libri au rapport de M. Boucly* (Paris, 1848), p. 87; (London, 1848), p. 62.

61 Libri, *Lettre à M. de Falloux.* Paris, 1849, pp. 76, 312.

who kept them only a few years. His collection[62] was bought secretly in 1847[63] by Bertram, 4th earl of Ashburnham (1797-1878),[64] the catalogues of whose MSS listed two volumes of Giordano's sermons as "Libri 532, 533."[65] The Libri portion of the Ashburnham MSS was acquired in 1884 by the Italian government, which assigned numbers 464 and 465 to Giordano's sermons (some of the Libri MSS having been restored to France).[66] The Mediceo-Laurentian Library was chosen as the most appropriate repository, and thus after an absence of four decades Pandolfini's MS of Giordano's sermons returned to Florence, where it is now called "Ashburnham 533, formerly 465."[67]

This brief glance at its history and contents is enough to show that Pandolfini played no Redi-like tricks in his quotation from Giordano. May we not even presume that it was he who first called Dati's attention to the passage in Giordano's sermon which is still our most important source for dating the invention of eyeglasses?[68] The statement that "It is not yet twenty years since there was found the art of making eyeglasses" forms part of a Lenten

62 P. L. Jacob, bibliophile (one of the pseudonyms of Paul Lacroix). Catalogue des manuscrits acquis par G. Libri. *Le bibliophile belge*, series 3, 1872, *7*, 91-103; the two volumes of Giordano's sermons which were bought by Libri from Pucci are listed on p. 113. Libri's handwritten catalogue of his MSS is now in the Mediceo-Laurentian Library, MS Ashburnham Appendix 1854.

63 Not "1846," as in the *Catalogo della libreria Pandolfini*, pp. 12, 17.

64 Delisle. Les manuscrits du comte d'Ashburnham. *Bibliothèque de l'École des chartes*, 1883, *44*, 204-206. *Catalogue des manuscrits des fonds Libri et Barrois*. Paris, 1888, pp. xxiii-xxvi. The earl's name was not "Askbaretam," as Greeff jumbled it (*Z. ophthal. Opt.*, 1918, *6*, 10).

65 *Catalogue of the manuscripts at Ashburnham Place*. London, n.d., part 1. *Eighth report of the royal commission on historical manuscripts*, appendix, part 3. London, 1881, p. 50. Since No. 533 was described as having 258 numbered leaves, Pandolfini's MS, which has only 161 leaves, must have been No. 532.

66 *Raccolta degli atti stampati per ordine della camera. Camera dei deputati, legislatura XV, sessione 1882-86*, Rome, 1886, *17*, no. 225, p. 30; or *Ministero della pubblica istruzione, Bollettino ufficiale*, 1884, *10*, 559. Since No. 465 was the 258-leaf MS, Pandolfini's MS must have been No. 464.

67 In the process of transfer from England to Italy, the Ashburnham numbers for the two Giordano MSS were interchanged. The English catalogues called Pandolfini's MS No. 532, and the 258-leaf MS No. 533 (see n. 65, above). The Italian official lists assigned No. 464 to Pandolfini's MS, and No. 465 to the 258-leaf MS (see n. 66, above). Hence arose the designations "532, formerly 464" (for the Pandolfini MS), and "533, formerly 465" (for the 258-leaf MS). But in the Mediceo-Laurentian Library the shelf mark "Ashburnham 532, formerly 464" denotes the 258-leaf MS, and "Ashburnham 533, formerly 465" Pandolfini's MS. *I codici Ashburnhamiani della Biblioteca Medicea Laurenziana di Firenze* (Ministero della pubblica istruzione, Indici e cataloghi, vol. 8, edd. Cesare Paoli, Enrico Rostagno and Teresa Lodi, Rome, 1888-1948) has not yet reached Pandolfini's MS.

68 Redi-Dati No. 1 shows that while reading the *Veglia* to Crusca, Dati dated the invention a little before 1300. Hence in May, 1673, he had already heard from Pandolfini about the passage in Giordano's sermon. Writing from Pisa on 5 March 1674, Redi sent to Dati the entry in the *Chronicle* about Giordano, with an apology for having previously overlooked it, presumably in Redi-Dati No. 2 and No. 3 (*Opere*, vol. 5, pp. 84-86). In short, Dati learned about Giordano's sermon from Pandolfini, before the latter died in 1655; and from Redi about the Pisan *Chronicle* and *Annals* not until November, 1673–March, 1674.

sermon[69] delivered by Giordano[70] at Florence[71] in the church of Santa Maria Novella on Wednesday morning, 23[72] February[73] 1305[74] (Florentine style, or 23 February 1306, common style). This is the principal reason for dating the invention shortly after 1286.[75]

Who was the Lettore?

At the point where Pandolfini's quotation from the sermon stopped, the text continues as follows:[76]

So short a time is it since there was invented a new art that never existed. And the *lettore* said, "I have seen the man who first invented and created it and I have talked to him."

The second of these two sentences "has no place in this sermon," wrote Manni, on the supposition that it was originally a marginal note and was later incorporated in the text by a foolish copyist (*Ragionamenti,* pp. 116-117). Manni's notion was uncritically accepted by Albertotti, who declared that the sentence "can be considered a gloss."[77] Insofar as the words "And the *lettore* said" interrupt the flow of the sermon, they resemble a gloss. But the purpose

69 Narducci, Enrico. *Tre prediche inedite del b. Giordano da Rivalto* (cited hereafter as "Narducci, *Tre prediche*"). Rome, 1857, p. 55. Offprinted from *Giornale arcadico di scienze, lettere ed arti,* 1857, *146,* 121. Narducci repeated Libri's mistaken complaint about the Milan edition of Redi's *Opere* (see n. 47, above), and also followed Muratori's erroneous ascription to the *Chronicle* of the Spina passage in the *Annals* (see n. 29, above; *Tre prediche,* pp. 21, 23; *Giornale arcadico,* pp. 87, 89).

70 The statement was not made in 1305 by Pandolfini (1575-1655), despite Masson, *France méd.,* 1908, *55,* 201. More than three centuries after the statement was made in 1306 by Giordano, it was quoted by Pandolfini.

71 Not Piacenza, as in Cohn, Hermann. Zur Geschichte der Brillen. *Gartenlaube,* 1895, p. 367. This error by the professor of ophthalmology at the University of Breslau was promptly echoed by Beckman, L. The invention of spectacles. *Opt. J.,* 1895-96, *1,* 126. The Cohn-Beckman confusion of the city where Giordano preached with the city where he died (see n. 139, below) was repeated by Feldhaus, *Die Technik der Antike und des Mittelalters.* Potsdam, 1931, p. 309, and also by Sarton, *Introduction,* vol. 2, p. 1025. Piacenza, where Giordano died, was transformed into his birthplace by Horner, Johann F. H. Ueber Brillen, aus alter und neuer Zeit. *Neujahrsblatt zum Besten des Waisenhauses in Zürich* (*Neujahrsblätter der Chorherrenstube,* no. 107). 1885, *48,* 10. Horner's blunder was copied by Bock, *Die Brille und ihre Geschichte,* p. 10; Golesceano, *Clin. prat. méd.-chir.,* 1914, *10,* 29; Singer, *Studies in the history . . . of science,* vol. 2, p. 399; and Reis, *Folia ophthal. orient.,* 1932-34, *1,* 399.

72 Not "28," as in Thompson, *Connoisseur,* 1934, *94,* 232.

73 This Lenten sermon was preached neither in January (despite Rouyer, *Coup d'oeil,* p. 20) nor in November (despite Davidsohn, Robert. *Geschichte von Florenz.* Berlin, 1896-1927, vol. 4, part 2, p. 12). Davidsohn's error was repeated by Von Lippmann, Edmund O. *Mitt. Gesch. Med. Naturw.,* 1927, *26,* 278. Lippmann also termed the Dominican monastery of St. Catherine a nunnery (because its patron saint was a woman?), located it in Florence instead of Pisa, made it the repository of Giordano's sermon instead of the *Chronicle,* and called the preacher "Rivolta."

74 Not 1300, as in Golesceano, *Clin. prat. méd.-chir.,* 1914, *10,* 29, and Greeff, *Dtsch. opt. Wschr.,* 1922, *8,* 602. Although Albertotti gave the date of the sermon correctly in 1914 (p. 347, no. 2), in later years (*Atti Accad. Padova,* 1922-23, *39,* 233; *Ann. Ottalm.,* 1923, *51,* 684; 1924, *52,* 276) he assigned it to 1317, by confusion with another (supposed) event connected with the invention of eyeglasses (cf. n. 161, below). By another confusion the year 1305 was assigned to the *Chronicle* by Greeff (*Erfindung,* p. 113), followed by Garrison (*Introduction to the history of medicine,* 4th ed., 1929, p. 185).

of a gloss is to explain some difficulty or obscurity in a text. "And the *lettore* said" should rather be regarded as a narrative formula.

How does it happen that such narrative formulas sometimes occur in Giordano's sermons as these have come down to us? It was not his habit to elaborate a discourse in all its details and commit it to writing before delivering it, as Manni once mistakenly thought.[78] Instead Giordano spoke extemporaneously on a theme about which he had meditated. His usual procedure is reflected in what he told the audience on two occasions about his own performance:

I thought of preaching to you not about that, but about something else; but when it pleased Him[79] that I should be so fluent[80] in this, thank God this has really been a good sermon. But anyway I want to tell you a little about what I had planned to say.[81]

75 The order of events was reversed (the invention being dated in 1305, and Giordano's sermon in 1285) by Gordon, *J. med. Soc. N. J.*, 1951, *48*, 5. Davidsohn remarks (vol. 4, part 2, p. 12) that ground or polished (geschliffenen) crystal was used before 1286 to read the epitaph on a spurious relic. But the Latin chronicle from which he professes to be quoting contains nothing corresponding to "geschliffenen." What the chronicler actually said was that "the epitaph could hardly be read with a crystal on account of the age of the writing." He undoubtedly referred to a magnifying glass made of crystal, not to a pair of eyeglasses. Davidsohn dated the episode about 1285, whereas his source put it in 1283, itself an error for MCCLXXX, because the "III" properly designated the day of the month; see *Monumenta Germaniae historica*, scriptores, vol. 32 (Hanover and Leipzig, 1905-13), pp. 520, 685. The chronicler does not say or imply that he personally used the crystal, despite Albertotti, Altri dati riguardanti la storia degli occhiali. *Atti Soc. Med.-chir. Padova*, 1923-24, 2, 174 (with a facsimile of the MS passage in table 1) and Von Lippmann, *Mitt. Gesch. Med. Naturw.*, 1927, *26*, 278-279. Eyeglasses may have developed from such aids to vision, as was suggested by Hallauer, Otto. Die Brille 100 Jahre vor und 100 Jahre nach der Erfindung der Buchdruckerkunst. *Universitätsaugenklinik und Augenheilanstalt Basel 1864-1914*. Basel, 1915, pp. 126-127, rather than from the ornaments and small window panes proposed by Ronchi. *Galileo e il cannocchiale*. Udine, 1942, p. 39. Perchè non si ritrova l'inventore degli occhiali? *Riv. oftal.*, 1946, *1*, 142-143. Storia delle lenti. *Atti d. fond. G. Ronchi*. 1947, 2, 4. *Occhi e occhiali*. Bologna, 1948; 2d ed., 1951, pp. 1-2. Ronchi was confident the inventor found eyeglasses by chance while looking for something else (as John Stuart Mill said happiness is found). But if so, why were eyeglasses found about 1286, not long after Ibn al-Haitham's *Optics*, the greatest Muslim treatise on the subject, had been made available in Latin translation, evoking discussions by Grosseteste, Roger Bacon, Witelo and Peckham? Two of these writers had emphasized the possibility of improving human vision by utilizing the refractive properties of transparent media. To be sure, they did not correctly understand the process of vision in the normal human eye, let alone the farsighted eye of the aged, nor did they know the true law of refraction. Therefore we can readily agree with Ronchi that the invention did not result from the application of sound theoretical principles. But this conclusion is by no means tantamount with invoking the operation of pure chance. May we not envisage an experienced glass-worker whose imagination was stirred by theoretical writings which, although not successful in solving the problem, at least pointed out the direction where the solution lay?

76 Narducci, *Tre prediche*, p. 60; *Giornale arcadico*, 1857, *146*, 126.

77 1914, p. 354, no. 6. Lettera intorno alla invenzione degli occhiali, *Ann. Ottalm.*, 1922, *50*, 85-86 (cited hereafter as "Albertotti, 1922"). Manni, on the other hand, was careful to say that the "marginal note" came from Giordano's own lips (*Ragionamenti*, pp. 116-117).

78 *Ragionamenti*, p. 117. In later years, when he became better acquainted with Giordano's sermons, he abandoned this erroneous conception (Manni, p. 73). Yet it was repeated by Pansa, Giovanni. L'invenzione degli occhiali. *Il Bibliofilo*, 1886, *7*, 17. Pansa also attributed the *Annals* passage about Spina to the *Chronicle* (cf. n. 29, above).

79 Reading "lui" (with Galletti, *L'eloquenza*, p. 161) instead of "lei."

I am so full, and I have so much in me, and I am so rich that I do not know what to say to you. I have said nothing of what I prepared, not a thing.[82]

Giordano's utterances were preserved (and thereby became the oldest extant specimens of preaching in the Italian language) because they were taken down by one or more devoted hearers.[83] Thus the MSS[84] of Giordano's sermons contain statements like these:[85]

At this sermon I was not present, except toward the end; I shall write down what I heard.

I arrived when he had already half finished.

I was not present at the beginning. . . . Up to this point I was not present, but I heard some few words of it repeated, and I have given them above. From here on I heard it.

This sermon was better organized and longer; this is just a summary; I was not there, but I got it by word of mouth from several who were present.

In this sermon Friar Giordano told many stories, both old and new, which I have omitted for the sake of brevity.

This was a long sermon, but it is written here in abbreviated form.
Of this sermon I write only a summary.

At these learned sermons a very great crowd was present.

It was these self-appointed scribes[86] (as we may call them) who introduced narrative formulas in their versions of the sermons, most frequently in the form "Friar Giordano said . . ." The concluding sentence of our sermon, for example, is preceded by the formula "And therefore Friar Giordano said . . ."[87]

But who is the *lettore?* Unfortunately this reference was misunderstood by Girolamo Tiraboschi, the first historian of Italian

[80] Reading "scorso" (not "scarso," as in Galletti, p. 161, and in Mioni, Ugo. *Il b. Giordano detto da Rivalto.* Pisa, 1927, p. 28).

[81] *Prediche del b. F. Giordano da Rivalto,* edited by Anton Maria Biscioni (see p. xxxxv) with an introduction by Domenico Maria Manni (cited hereafter as "Manni-Biscioni"). Florence, 1739, p. 121. Manni's introduction was reprinted in vol. 1, pp. xix-xlviii, of Giordano's sermons when they were included in the *Biblioteca scelta di opere italiane antiche e moderne* (vol. 384-386, Milan, 1839).

[82] *Biblioteca scelta,* vol. 386, p. 60.

[83] Galletti, pp. 150-151. Earlier sermons delivered before laymen in the language of the people have survived, if at all, only in abridged Latin translations. Galletti, pp. 59-60, 68-70, 91-95.

[84] These are more numerous than the MSS of any other Italian preacher, including Savonarola. Galletti, p. 150.

[85] Manni-Biscioni, p. 106; *Prediche del b. Fra Giordano da Rivalto,* ed. Domenico Moreni, Florence, 1831 (cited hereafter as "Moreni"), vol. 1, p. 44; vol. 2, pp. 77-78; Manni-Biscioni, pp. 193, 192, 297, 303; Moreni, vol. 2, p. 238; *Biblioteca scelta,* vol. 384, p. 169; vol. 386, p. 155; vol. 385, pp. 282-283; vol. 384, pp. 358, 355; vol. 385, pp. 116, 127, 359. Cf. Maggini, Francesco. Saggi di prediche inedite di Giordano da Rivalto. *Studi di filologia italiana.* 1938, 5, 30.

[86] I say "scribe" in deliberate opposition to the use of "copyist" by Manni, who for a time mistakenly supposed that Giordano was the first to write down the texts of his sermons (cf. n. 78, above).

[87] Narducci, *Tre prediche,* p. 67; *Giornale arcadico,* 1857, *146,* 133.

literature, who remarked: "Whoever took down the sermon from Giordano's mouth, and wrote it out, could easily have known the inventor and talked to him."[88] Yet there is no lexicographical warrant whatever for equating *lettore* with "scribe." On the other hand, a MS containing our sermon bears the following title: "Sermons of Friar Giordano of the Dominican Friars when he was in Florence as *lettore* [teacher] of the Friars."[89] From this MS, Biscioni, the earliest editor of the sermons, took the first of his two groups of Giordano's discourses, and printed the title of the MS in two places, while Manni specifically identified Giordano as the *lettore*.[90] All this was overlooked by Tiraboschi, perhaps a pardonable oversight on the part of a pioneer who undertook to discuss a vast number of authors and subjects. But when the *Chronicle* was published in 1845, it made known (p. 451) to everyone interested in Giordano that he lectured on theology "in the general seminary [conducted by the Dominican order in the monastery of Santa Maria Novella] in Florence; then for three years he was principal *lector* in the same place."[91] Despite the evident equivalence of Latin *lector* with Italian *lettore*, the Giordano specialist who edited our sermon repeated Tiraboschi's error.[92] Then the minutes were published of a meeting in 1305 of Giordano's provincial chapter.[93] At that meeting a certain friar was assigned to teach at the seminary in Florence with the proviso that if he could not

88 *Storia della letteratura italiana*, tome 4, part 1, book 2, chapter 2, no. 26. Ed. Modena, 1787-94, vol. 4, p. 199. Tiraboschi's remark was quoted with approval by Rambelli, Gianfrancesco. *Intorno invenzioni e scoperte italiane*. Modena, 1844, p. 137.

89 *I manoscritti della r. Biblioteca Riccardiana di Firenze* (Ministero della pubblica istruzione, Indici e cataloghi, vol. 15, ed. Salomone Morpurgo, Rome, 1893-1900), no. 1268, pp. 329-330. Cf. Moreni, vol. 2, p. 326; Narducci, *Prediche inedite*, p. xlii; and Davidsohn, vol. 4, part 3, Anmerkungen, p. 16. Narducci's edition of our sermon (*Tre prediche*, pp. 18, 55-67; *Giornale arcadico*, 1857, *146*, 84, 121-133) was based on this MS Riccardiano 1268, fol. 135*r*-139*r*; the passage about eyeglasses begins on fol. 136*v*, column 2, and runs over to fol. 137*r*, column 1 (facsimile in Albertotti, 1914, p. 338, fig. 3).

90 Manni-Biscioni, pp. xxx, xxxiv, 1. *Ragionamenti*, pp. 115, 116, 119. Manni, pp. 72-73. *Biblioteca scelta*, vol. 384, pp. xxv, xxxvi.

91 Cf. *Chronicle*, p. 401. In an unpublished sermon of 4 March 1305 (Florentine style; 1306, common style; "1365" is a misprint) Giordano reminded his listeners: "I have preached to you three years already" (*Giornale storico della letteratura italiana*, 1898, *31*, 18; 1899, *33*, 195). His sermons range in date from January 1303 to September 1306 (with two more in 1309, when he returned to Florence for a meeting of his provincial chapter). He did not "find himself occasionally in Florence for the purpose of preaching," as Albertotti mistakenly thought (1914, p. 339). *Biblioteca scelta*, vol. 384-386, reprinted the sermons in chronological order. Those preached in 1306 were overlooked by Orlandi, *Necrologio*, vol. 1, p. 495.

92 Narducci (*Tre prediche*, p. 25; *Giornale arcadico*, 1857, *146*, 91), followed by Martin, Thomas-Henri. Sur des instruments d'optique faussement attribués aux anciens. *Bullettino di bibliografia e di storia delle scienze matematiche e fisiche*, 1871, *4*, 237.

93 Masetti, *Monumenta . . . ordinis praedicatorum*, vol. 1, p. 304, mistakenly called the meeting a *general* chapter, although he included the meeting at Rieti on 13 September 1305 in his list of provincial chapters (vol. 1, p. 41). Hence for Davidsohn (vol. 4, part 3) it was both a general chapter (p. 69) and a provincial chapter (Anmerkungen, p. 16).

carry out the assignment, "we appoint Friar Giordano of Pisa *lector* in the same seminary."[94] An ex-Jew who had accepted conversion to Christianity (the only alternative being death) was called by Giordano

"my associate friar, and I have traveled with him many times, and he was a learned man and he was *lettore* in Naples" [Moreni, vol. 2, p. 231].

Yet Albertotti (1922, pp. 85-86), an historian of eyeglasses, persisted in accepting the Tiraboschi-Narducci error, thereby ignoring the decisive evidence that had accumulated in support of the sound interpretation originally put forward (Manni, p. 74) and subsequently reiterated (Tempesti, pp. 60-61). Therefore when we read in our sermon that

the *lettore* said "I have seen the man who first invented and created it [the new art of eyeglasses] and I have talked to him"

we may with complete confidence reject the Tiraboschi-Narducci-Albertotti error and hold that the quoted statement was made by Giordano himself, not by any scribe, as an integral part of his discourse and not as a gloss (still less, part of a gloss).

The preacher was designated by name instead of by his office in another sermon (Narducci, *Prediche inedite,* p. 183), in which he talked about a noteworthy object at Milan and three at Cologne:

And Friar Giordano said "I have seen it" . . . and Friar Giordano said "I have seen them."

Of a form strikingly similar to our whole passage is the account in still another sermon (Moreni, vol. 2, pp. 227-228) of the killing of over 24,000 Jews in one phase of the Rindfleisch pogroms:

It is not yet four years since this took place in Germany. And Friar Giordano said: "I was in that region when it happened . . ."

The purport of our whole passage was misinterpreted by Tempesti, who thought (p. 61) that it "announced from the pulpit, as important news (pubblicare dal pulpito, come notizia di rimarco), the new art of making eyeglasses." True, Giordano did use the expression "new art that never existed" (arte novella che mai non fu). But he said it had been invented nearly twenty years before. In other words, the new art had long ceased to be news, in any

[94] Masetti, vol. 1, p. 131; improved reading at vol. 1, p. 304. Note the contemporary designation "Giordano of Pisa" (cf. n. 52, above, and Narducci, *Prediche inedite,* p. xxxviii); he was known by no other name to a historian of the Dominican order as late as 1517. Alberti, Leandro. *De viris illustribus ordinis praedicatorum.* Bologna, 1517, p. 226*r*. At this same meeting of 1305 the initiator of the *Chronicle,* Bartolomeo da San Concordio, was assigned to the Dominican monastery at Arezzo (Masetti, vol. 1, p. 131). Cf. Thomas Kaeppeli, "Acta capitulorum provincialium provinciae romanae," *Monumenta ordinis fratrum praedicatorum historica,* 1941, *20,* 154.21-25.

proper sense of that term. Giordano's purpose in referring to the invention was not, as Tempesti mistakenly thought, to announce important news. He cited it rather as an example of the unfailing ingenuity of mankind:[95]

All the arts are found by man. Man doesn't make them, but he finds them; and yet they haven't all been found. The finding of arts will never come to an end. Any day one of the arts could be found. Does the jester mention[96] them all in the song? He doesn't mention[96] them all, because in the world there are plenty of them that he doesn't know. From that song are missing many arts that are carried on beyond the mountains. Up there in Paris is a great art of carving and cutting precious stones, which is a great art there. And thus throughout the world there are many of them that you don't know, and yet they haven't all been found.[97] Many of them have been found,[98] but any day a new one could be found, and new ones are always being found.[99] It is not yet twenty years since there was found the art of making eyeglasses. . . .

Anyone still inclined to regard our fourteenth-century ancestors as indifferent to innovation would do well to ponder the following pronouncement by Giordano (Manni-Biscioni, p. 26):

Familiar things which man knows, he ordinarily cares little about and has little desire for them; but should there be something new and unfamiliar, people usually want very much to know it.

This avid public interest in novel things will later be seen to have an important bearing on the question where eyeglasses were invented.

Redi's "Lettera"

But now we should examine how Redi's *Lettera* treated Giordano's sermon about eyeglasses. Like Pandolfini in Dati's *Veglia*, Redi quoted only this portion (*Opere*, vol. 2, p. 261):

It is not yet twenty years since there was found the art of making eyeglasses which make for good vision, one of the best arts and most necessary that the world has.

He did not quote what immediately follows:

95 Narducci, *Tre prediche*, pp. 59-60; *Giornale arcadico*, 1857, *146*, 125-126. Feldhaus was utterly mistaken when he said (*Technik der Antike*, p. 309) that Giordano praised the invention of eyeglasses as a "divine gift."

96 Reading "mentova" (not "trova," as in Manni, p. 73). The jester's song has been preserved, despite Davidsohn, vol. 4, part 2, p. 12; see Pio Rajna, *Zeitschrift für romanische Philologie*, 1881, 5, 30-40, and De Bartholomaeis, Vincenzo. *Rime giullaresche e popolari d'Italia*. Bologna, 1926, pp. 17-20.

97-98 The material between these two numbers is lacking in Manni, p. 73.

99 Yet Pansa could say that this text discusses art and not invention (*Bibliofilo*, 1886, *7*, 17). Consistency is a virtue hardly to be expected in a popular preacher like Giordano. In a sermon delivered on 14 February 1304 (Florentine style; 1305, common style), about a year before ours, he said: "Today every common man is enlightened. There are so many books and wise things written down as to be without number. Our ignorance is due only to negligence in not reading. Everything has been worked out for us. Nothing remains to be said. Hence today a new book couldn't be made, nor a new art" (Manni-Biscioni, p. 152).

So short a time is it since there was invented a new art that never existed. *And the teacher said,* I have seen the man who first invented and created it and I have talked to him.

Why did he omit this continuation?

Before attempting to discover the true reason, we must dispose of a specious explanation. The continuation was first published,[100] half a century after Redi's *Lettera,* by Manni, who insisted that Redi never saw these words.[101] Some sixty years later the passage was said to have been taken by Manni "from a more obscure MS" (Tempesti, p. 60), and to have been "dug out of another MS" (Canovai, *Memorie,* vol. 2, p. 244). Both these writers implied that the continuation was lacking in the Giordano MS known to Redi. In their time such a misconception may have been excusable. But Albertotti[102] also said of Manni that "he takes advantage of a passage in other MSS" than the one used by Redi.[103] Yet in the *Lettera* (*Opere,* vol. 2, p. 261) Redi quoted from the "sermons of Friar Giordano da Rivalto in Filippo Pandolfini's MS." And Albertotti personally examined Pandolfini's MS (now Ashburnham 533) at the Mediceo-Laurentian Library in Florence and copied the continuation from it.[104] Redi's reason, therefore, for omitting the continuation from his *Lettera* was not its unavailability to him as Manni, Tempesti, Canovai, and Albertotti mistakenly supposed. He had the passage before his eyes in Pandolfini's MS and deliberately excluded it from the *Lettera.*

He also pretended in the *Lettera* not to know the exact date of our sermon. After quoting Giordano's statement that the invention of eyeglasses was less than twenty years old, and pointing out that the preacher died in 1311, Redi concluded (*Opere,* vol. 2, p. 262):

Therefore, it seems to me, I can honestly (ingenuamente!) declare that the art of making eyeglasses is a modern invention and was discovered in Tuscany[105] in the years which ran, to state it broadly, from 1280 to 1311.[106] And this period could be narrowed down even more if it were known or could be guessed in what year Friar Giordano delivered the sermon. In some MSS I have found it written among those which he preached in Florence about 1305.

[100] *Ragionamenti,* p. 116; Manni, p. 73.

[101] *Ragionamenti,* p. 118.

[102] 1914, p. 338; followed by the editor of *Riv. Stor. crit. Sci. med. nat.,* 1914, *5,* 321; by Greeff, *Z. ophthal. Opt.,* 1919, *7,* 7; *Erfindung,* p. 75; and by Del Guerra, *Ann. Ottalm.* 1933, *61,* 384. *Riv. Stor. crit. Sci. med. nat.* 1933, *24,* Atti, p. 18.

[103] Albertotti (1914, p. 337) applied the term "variant" to Manni's quotation as compared with Redi's. But this is no matter of mere variants. Redi ended his quotation where it suited his purpose to do so. Manni continued from that point for a reason of his own, as we shall see in the text below, at n. 214.

[104] 1914, p. 356. Yet he could say (*Atti Accad. Padova,* 1913, 14, *30,* 312) that Manni's MS differed from Dati's (which was Pandolfini's).

Hence Redi consulted some Giordano MSS in addition to Pandolfini's. And it was the uniform practice of the scribes to prefix to each sermon the time and place of its delivery, specifying year, month, day (and sometimes part of the day, as in the case of our sermon). For this is headed, in Pandolfini's MS (fol. 55v), "23 February 1305, Wednesday morning."[107] Redi's indefinite reference to the sermons preached by Giordano "about (intorno al) 1305" was nothing but sheer obfuscation. When he wrote ". . . if it were known or could be guessed in what year Friar Giordano delivered the sermon" about eyeglasses, he knew precisely not only the year but the month and the day, and very nearly the hour.

Casting a veil of uncertainty over the chronology of the sermon tended to diminish its importance. So did the omission of Giordano's declaration that he was personally acquainted with the inventor. By deleting this declaration, Redi left out of his *Letter concerning the invention of eyeglasses* what amounts to half of our source material about the inventor of eyeglasses. His motive for so drastic a suppression may be discerned, I believe, by examining how he treated the other half of our sources about the inventor, namely, the Spina passage in the Pisan *Chronicle* (pp. 476-477):

> Friar Alessandro della Spina was a modest and good man. Whatever he saw or heard had been made [the *Chronicle* actually says: "Whatever had been made, when he saw it with his own eyes"][108] he too knew how to make

[105] Instead of "Tuscany," "Florence" appears in the French translation of Redi's *Lettera* by Spon, Jacob. *Recherches curieuses d'antiquité*. Lyon, 1683, p. 217. The substitution may involve nothing more than an inadvertent slip, however, for the claim that eyeglasses were invented in Florence was first put forward in 1684 (see n. 153, below). Spon's *Recherches* were not published in 1684, as in the *Catalogue général des livres imprimés de la Bibliothèque nationale. Auteurs.* Vol. 176 (Paris, 1949), col. 863-864; nor 1693, as in Pansier (*Hist. des lunettes*, p. 1); nor 1681, as in Greeff (*Z. ophthal. Opt.*, 1918, *6*, 3; *Erfindung*, p. 67). It was a letter by Redi, not by Dati (as in Martin, p. 235; see n. 92, above) that was inserted in the *Recherches* by Spon. His name was corrupted to "Spoon" by Smith, Robert. *A compleat system of opticks.* Cambridge, 1738, Remarks, p. 12. Smith's "Spoon" was used also by Fox, L. Webster. A history of spectacles. *Medical and surgical reporter* (Philadelphia), 1890, *62*, 514.

[106] The same statement was made by Redi (*Opere*, vol. 8, p. 111) in a letter whose date is missing. It was supplied as 1670 by Rocchi, *Riv. Stor. crit. Sci. med. nat.*, 1913, *4*, 4. But he overlooked the editor's comment, which serves to fix the time of composition as the second half of 1678. For in the letter Redi remarked: "this year it has pleased these gentlemen to make me Archconsul of the Accademia" della Crusca, an office to which he was elected on 27 June 1678 (*Opere*, vol. 8, p. 113). He also disparaged the examples of his own prose and poetry which his correspondent, Gilles Ménage, had included in the *Mescolanze*, published in 1678. Moreover Redi announced that "here in Florence within two or three months they will finish printing the sermons of Father Paolo Segneri," whose *Quaresimale* (or Lenten sermons) was published in 1679, the dedication being signed 15 April. Finally Redi said: "On my return to Florence, which will be perchance within a few days, I shall publish a letter about the inventor of eyeglasses and the period in which they were invented." Redi's *Lettera* was indeed issued (as we saw in n. 47, above) in 1678. Hence, for these four reasons, we may confidently discard the date (1670) assigned to the Redi-Ménage letter by Rocchi who, incidentally, misquoted Redi's delimitation of the period of invention as 1280-1331 instead of 1280-1311.

[107] Same heading in MS Ashburnham 532, fol. 127 *r*; MS Riccardiano 1268, fol. 135*r*; and BNC MS Palatino 113, fol. 17*v*.

it. Eyeglasses were first made by someone else,[109] who was unwilling to share them. Spina made them,[110] and shared them with a cheerful and willing heart.[111]

Immediately after this quotation (and misquotation) of the *Chronicle,* Redi said (as we have already seen in connection with the analogy he drew between Spina and Galileo):

From this it follows that if Friar Alessandro Spina was not the first inventor of eyeglasses, at least it was he who by himself without any instruction rediscovered (rinvenne) the method of making them; and that at the very time when he lived, this most useful invention first came to light.

From the *Chronicle's* categorical statement that "eyeglasses were first made by someone else" ("aliquo" is Redi's reading; "alio" is the correct reading,[112] the difference being of slight importance for our present discussion) Redi deduced the astonishing protasis: "if Friar Alessandro Spina was not the first inventor (primo inventore) of eyeglasses." Before long, this protasis ceased to be negative and ceased to be a protasis, for Redi soon spoke of "Friar Alessandro Spina, inventor (trovatore) of eyeglasses."[113] Thus by three easy stages he passed from quoting the *Chronicle* to contradicting it.

[108] Despite Davidsohn's statement (vol. 4, pt. 2, p. 13) that he was translating from the *Chronicle,* in fact he followed Redi's falsified version. For, according to Davidsohn, the *Chronicle* said Spina could reproduce the things "of which he heard or which he saw."

[109] The *Chronicle* does not say when. It does not date the invention in 1305, despite Gordon (*J. med. Soc. N. J.,* 1951, *48,* 5).

[110] Davidsohn translated: "die Brille, die ein anderer erdacht, der ihm das Geheimnis ihrer Herstellung aber nicht verraten wollte, habe er nacherfunden." But the *Chronicle* contains nothing corresponding to "ihm." By introducing this word, Davidsohn made the *Chronicle* say that the inventor refused to tell Spina about eyeglasses (cf. vol. 4, pt. 2, Anmerkungen, p. 25). Actually the *Chronicle* implies that the inventor would not talk about them to anybody, including Spina. The statement that "Spina . . . had a pair of spectacles made for himself by an optician who had the secret of their invention" (L. W. Fox, *Medical and surgical reporter,* 1890, *62,* 514) is not based on the *Chronicle* or on any other source.

[111] Redi, *Opere,* vol. 2, pp. 259-260. It was not Spina who "kept the process a secret," despite Trestelle (pseudonym of Giulio Martinez). Sulla storia degli occhiali. *Boll. Ass. ottica ital.,* 1941, *15,* 8.

[112] Nevertheless "aliquo" appears in the *Enciclopedia italiana,* vol. 25, p. 115.

[113] *Opere,* vol. 2, p. 261. Hirschberg (*Geschichte der Augenheilkunde,* vol. 13, p. 272) erred in saying that Redi does not speak of the inventor. Redi's characterization ("Spina, inventor of eyeglasses") was misleadingly reported by Albertotti (1914, p. 338), according to whom Redi said "Spina was an inventor of eyeglasses." On the other hand, Redi was faithfully followed by a historian of the Archiepiscopal Seminary of Pisa who listed "Alessandro della Spina, inventor of eyeglasses" among the illustrious men of the monastery of St. Catherine. Zucchelli, Niccola. *Appunti e documenti per la storia del seminario arcivescovile di Pisa.* Pisa, 1906, p. 188. This utterly indefensible position was adopted also by Giovanni Battista Picotti in the *Enciclopedia cattolica,* Vatican City, 1949-54, vol. 9, col. 1565. Vincenzo Marchese changed his mind. In the first edition (Florence, 1845-46) of his *Memorie dei più insigni pittori, scultori e architetti domenicani,* although he quoted the *Chronicle* passage about Spina in an unadulterated form, he nevertheless repeated Redi's "Spina, inventor of eyeglasses" (vol. 1, p. 177; cf. C. P. Meehan's translation, *Lives of the most eminent painters, sculptors and architects of the order of S. Dominic,* Dublin, 1852, vol. 1, p. 121). But in his later editions (Florence, 1854, vol. 1, p. 153; Genoa, 1869, vol. 1, p. 228; Bologna, 1878-79, vol. 1, p. 205) Marchese, evidently realizing the contradiction between the *Chronicle* and Redi, abandoned the latter's unsound claim for Spina. The attribution of the invention to Spina in Marchese's first edition was overlooked by Del Lungo in his 2d ed., *Chi l'inventore degli occhiali?* Bologna, 1921, p. 13.

For according to the *Chronicle* somebody other than Spina invented eyeglasses; according to Redi's *Lettera*, Spina.[114]

In this conflict between Redi and his source, which version did his readers accept? According to a contemporary art historian, Redi "proves conclusively" (prova ad evidenza) that Spina invented eyeglasses;[115] "proves" was echoed by an encyclopedist who prepared the way for the first history of Italian literature.[116] Was the *Chronicle* similarly disregarded by other readers?[117] These two, whom Manni quoted (pp. 62-63) directly after his reprint of the *Lettera* (pp. 55-61), did not, he felt, "clearly express Redi's sentiment." But both were influential authors who repeated Redi's conclusion, itself distasteful to Manni, whose book was intended to glorify as the inventor of eyeglasses somebody other than Spina.

Redi's commitment to Spina was, I believe, the reason for the omission from the *Lettera* of Giordano's statement that he had talked to the inventor. For it would have been extremely difficult, even for a writer as artful as Redi, to convince readers that when Giordano referred to the inventor, he had Spina in mind. Before examining the precise character of the difficulty, however, let us first look at another aspect of Redi's treatment of Spina, namely, the date of his death.

When did Spina die?

In the *Lettera* Redi correctly assigned Spina's death to 1313.[118] But previously, in Redi-Dati No. 3 (*Opere*, vol. 7, p. 255), he had said that "Spina died either in 1312 or in 1313," a formulation that did no great harm. In Redi-Dati No. 2, however, he injected an element of confusion into the subject by asserting that "Spina died in the year 1313 Pisan style, and 1312 Roman style."[119]

114 According to Greeff (*Erfindung*, pp. 84, 94), followed by Weve, H. J. M. (Korte mededeeling betreffende een verdichte brillengeschiedenis. *Ned. Tijdschr. Geneesk.*, 1927, *71:1*, 3142), Spina, a Pisan, was made the inventor by Redi, because the latter wished to secure the invention for "Pisa, his native city." Redi was born at Arezzo.

115 Baldinucci, Filippo. *Vocabolario toscano dell' arte del disegno*. Florence, 1681, p. 109; reprinted in *Classici italiani*, vol. 191, Milan, 1809, p. 8.

116 Gimma, Giacinto. *Idea della storia dell' Italia letterata*. Naples, 1723, pp. 610-611.

117 Giving full weight to the *Chronicle*, Cancellieri said that Redi put Spina "among the first inventors" of eyeglasses. *Lettera . . . al . . . Koreff*, pp. 291-292.

118 Spina did not die in 1338, despite *Brit. med. J.*, 1907, *1*, 162.

119 *Opere*, vol. 5, p. 83. The whole letter was reprinted by Manni, pp. 53-54. The quoted statement was repeated by Tiraboschi, vol. 4, pt. 1, bk. 2, chap. 2, no. 25; Rambelli, *Intorno invenzioni e scoperte italiane*, p. 136; Narducci, *Tre prediche*, p. 23 (*Giornale arcadico*, 1857, *146*, 89); and Greeff, *Erfindung*, p. 78. Dating by two calendars being cumbersome, the less familiar Pisan style was dropped by certain writers, who accordingly put Spina's death (incorrectly) in 1312; see, e.g., *Ad nova acta eruditorum . . . supplementa*, 1749, *6*, 311; Tempesti, p. 59; Pansa, *Bibliofilo*, 1886, *7*, 18; Zucchelli, *Appunti*, p. 188; Horner, *Neujahrsblatt . . . des Waisenhauses in Zürich*, 1885, *48*, 10; Cohn, *Gartenlaube*, 1895, p. 367; Beckman, *Opt. J.*, 1895-96, *1*, 126.

It is in Redi-Dati No. 2 that he mistook the *Annals* for the *Chronicle*. Now in the *Annals*, which were written in 1550, almost a century and a half after the *Chronicle* came to an end, some events are dated in the Pisan style (*Chronicle*, pp. 606, 621, 622, 630) but others in the Roman (pp. 627, 628, 629), with the former apparently predominant in the mind of the annalist. He relates, for instance, that a certain friar, who became prior in 1522, after a few months in office died in the autumn. His successor "became our prior, in the same year, in the month of January" (*Chronicle*, pp. 625-626). This statement obviously presupposes the Pisan calendar, in which January followed autumn of the same year, because the Pisan New Year began on 25 March. Of course, in the Roman calendar, with 25 December marking the start of the New Year, January followed the autumn of the preceding year, and preceded the autumn of the same year. The *Annals*, then, were written basically in the Pisan style, with some intrusions of the Roman style.

No such alternation, however, exists in the *Chronicle*, which employs only a single chronological style, the Roman. It records, for example, "the year of the great plague, 1348, which wiped out almost the whole world. In that year there died in Pisa more than forty of the friars; and of the laity, a throng which nobody could count."[120] Two of the friars whose deaths the *Chronicle* lists in 1348 are mentioned also in a fourteenth-century register of property and revenue belonging to the monastery of St. Catherine. This register reports (*Chronicle*, pp. xx, 547) that one of the two died in "1349, Pisan style (secondo lo corso di Pisa), on the 10th or 11th day of June," and the other in "1349, on the 24th or 25th day of July at the time of the great plague epidemic." The year 1349, Pisan style, began 25 March 1348, Roman style, and ended 24 March 1349, Roman style. The deaths assigned by the register to the summer of 1349, Pisan style, are dated 1348 in the *Chronicle*,[121] which used the Roman style throughout.

Neither the *Chronicle* nor the *Annals* mentions the date of Spina's death.[122] How then could Redi, after blundering twice in his letters to Dati, infer correctly in the *Lettera* that Spina died in

120 P. 530. In the Introduction to the First day of the *Decameron*, in his graphic account of the 1348 plague, Giovanni Boccaccio says that between March and July "more than one hundred thousand human beings are believed for certain to have died within the walls of the city of Florence," while the friars were "reduced almost to nothing."

121 Hence we must reject as unsound the assumption that because the *Chronicle* was written in Pisa, it used the Pisan style. Mioni, *Il b. Giordano detto da Rivalto*, p. 61.

122 Despite Bock (*Die Brille und ihre Geschichte*, p. 10) and Barck (*St Louis med. Rev.*, 1904, *49*, 306; *Open court*, 1907, *21*, 213).

1313? Giordano is the first friar whose year of death is recorded in the *Chronicle* (p. 452). For the 106 friars preceding him this information is not given, nor for 89 following him (including Spina). It is given for the remaining 80, whose notices are generally arranged according to the year of their death,[123] starting with Giordano in 1311.[124] The chronological order, however, is not observed strictly. Thus no. 125 (†1328) comes between 1318 and 1320, no. 139 († May, 1324) comes after July, 1324 and 1327, while the displacement of no. 140 and 141 is attributed by a chronicler to an oversight on the part of his predecessors. Constituting a second disordered group, nos. 248, 250, 252, 255 are dated 1383, 1381, 1385, 1382. Thirdly, nos. 272, 273, 274 (1408-1409) belong between no. 269 (1407) and no. 270 (1409). This last departure from strict chronological order may be connected with the death of the third and principal chronicler, Domenico da Peccioli, in 1408,[125] and the resumption of the *Chronicle* by Simone da Cascina

[123] The *Chronicle* (p. 437) explicitly confirms that it proceeds in an orderly way.

[124] Because Mioni (cf. n. 121, above) mistook the *Chronicle's* style, he misdated Giordano's death in 1310, an error repeated in *Enciclopedia italiana*, vol. 13, p. 112. Giordano did not compose his sermons between 1330 and 1336, despite Amati (*Ricerche storico-critico-scientifiche*, vol. 4, p. 144), nor between 1300 and 1306, despite Pansa (*Bibliofilo*, 1886, *7*, 17). For the correct dates, see n. 91, above.

[125] Cf. n. 32, above. He died in December, 1408, according to the *Chronicle* (p. 589). Was it because of the error mentioned in n. 121, above, that Bonaini gave the year as 1407 (*Archivio storico italiano*, 1844, *6:1*, xxvii)? Galletti (*Giornale storico della letteratura italiana*, 1898, *31*, 2) followed Bonaini instead of the *Chronicle*, and in *L'eloquenza* (p. 243) Galletti gave the year as 1420.

[126] *Chronicle*, p. 582; hence he could not have died in 1407, the date given by Petella (*Ann. Med. nav. colon.*, 1901, *7:1*, 830). In 1414 he preached in Florence (Orlandi, *Necrologio*, vol. 2, p. 387) and he died about 1420 (*Archivum fratrum praedicatorum historicum*, 1942, *12*, 201-202). Galletti (*Giornale storico della letteratura italiana*, 1898, *31*, 2; *L'eloquenza*, p. 148) asserted that he continued the *Chronicle* up to the year 1409; but it records the death of no. 271 in 1410 (p. 586).

Redi confused our chronicler Simone da Cascina with a better known writer of similar name, the theologian Simone Fidati da Cascia (†1348). McNeil, Mary G. *Simone Fidati and his De gestis domini salvatoris.* Catholic University of America dissertation. Studies in medieval and renaissance Latin language and literature, vol. 21. Washington, D. C., 1950, pp. 35, 229. Despite Bonaini's warning (*Chronicle*, p. 599), Redi's "Simone da Cascia" was incorrectly identified by Petella (*loc. cit.*) with the relatively obscure author of a letter written in 1240 about an injury caused by looking directly at the sun during an eclipse. Puccinotti, Francesco. *Storia della medicina.* Livorno, Prato, 1850-66, vol. 2, pt. 1, p. xxix. Redi-Dati No. 3 transmitted the wrong name (*Opere*, vol. 7, p. 254) to Dati, who trustfully put the misnomer in the *Veglia* (Targioni, p. 60), while Redi put it in the *Lettera* (*Opere*, vol. 2, p. 259). His error was corrected in the *Notizie* (p. 25) which Manni wrote as an introduction to his edition (Florence, 1734) of Bartolomeo's *Ammaestramenti degli antichi* (reprinted in *Classici italiani*, vol. 21, Milan, 1808, p. xx, and in *Biblioteca scelta di opere italiane antiche e moderne*, vol. 231, Milan, 1829, p. xviii). Although Manni's correction was repeated by Bonaini (*Arch. stor. ital., 6:1*, xxix; *Chronicle*, p. 599), Redi's error recurs in Caverni, Raffaello. *Storia del metodo sperimentale in Italia.* Florence, 1891-1900, vol. 1, p. 501; Vitelli and Tamburini (cf. n. 9, above); Greeff, *Z. ophthal. Opt.*, 1917, *5*, 75; 1918, *6*, 4; *Erfindung*, pp. 77, 79; and Del Lungo, p. 21.

The *Chronicle* includes six members of the Da Cascina family besides Simone: Guglielmo, Marino, Tommaso, Francesco, Niccolò and Pietro, with the *Annals* contributing Giovanni (pp. 443, 485, 542, 552, 565-6, 597). Ranieri Segalorzo (p. 516) does not belong here, despite Zucchelli, p. 188, for the *Annals* (p. 45) say: "F. Rainerius Segalorzus Cascina oriundus fuit"; cf. Alberti, *De viris illustribus ordinis praedicatorum*, p. 227*v*.

in 1411.[126] The irregularities of the period 1381-1385 may have been caused by the absence of Domenico da Peccioli, who was preaching in Florence in 1382 and serving as provincial prior or vicar in 1385.[126.1] The only other group of deviations falls within the decade 1318-1328; was it then perhaps that the initiator of the *Chronicle,* Bartolomeo da San Concordio, abandoned it and turned to other occupations?

In the *Lettera* (*Opere,* vol. 2, p. 259) Redi said: "Friar Bartolomeo da San Concordio having died in 1347[127] . . . the *Chronicle* was continued by Friar Ugolino di ser Novi."[128] Redi was blamed by Manni[129] for imagining that Bartolomeo kept working at the *Chronicle* until his death in 1347. For his part Manni maintained that Bartolomeo "wrote this [*Chronicle*] up to the year 1314, when he lifted his hand from the work." But instead of evidence to support his statement Manni offered the mere conjecture that Bartolomeo "put the writing aside in the year 1314, perhaps on account of one of his journeys."[130] Is there any more reason to trust Manni in this matter than in the question of Bartolomeo's family? For when Manni professed to be quoting from the *Chronicle* passage about Bartolomeo, actually he was misquoting from the *Annals.* For where the *Annals* read "Bartholomeus a S. Concordio," Manni wrote "Bartholomeus Granchi a S. Concordio."[131] He mis-

126.1 Orlandi, *Necrologio,* vol. 2, pp. 58, 538. *Monumenta ordinis fratrum praedicatorum historica,* 1937, *19,* p. 62, no. 11; in 1386 he was no longer an official of the province (p. 63, no. 21).

127 Earlier, in Redi-Dati No. 3, he had incorrectly reported the year of Bartolomeo's death as 1341 (*Opere,* vol. 7, p. 255; cf. *Chronicle,* p. 524).

128 He was called "Servoni" by Rouyer, *Coup d'oeil,* p. 19, followed by Mme. Heymann, *Lunettes,* p. 5, and Reis, *Folia ophthal. orient.,* 1932-34, *1,* 399.

129 *Notizie,* p. 26, in his edition of Bartolomeo's *Ammaestramenti; Classici italiani,* vol. 21, p. xx; *Biblioteca scelta,* vol. 231, p. xix.

130 Manni asserted that two historians of the Dominican order had put Bartolomeo's death in 1314, and that "this error was noticed also by Francesco Redi." But Redi did not notice this (alleged) error; and what the historians actually said was correct, that Bartolomeo "flourished about 1314." Alberti, *De viris illustribus ordinis praedicatorum,* p. 151*r* (misnumbered p. 150). Razzi, Serafino. *Istoria de gli huomini illustri . . . de gli Predicatori.* Lucca, 1596, p. 267. Acceptance of 1314 as the date of Bartolomeo's death was attributed by Manni to Giovanni Cinelli too. But Cinelli, although leaning toward 1314, regarded 1347 as a possible alternative. *Storia degli scrittori fiorentini e toscani.* BNC, Magliabechi MS, cl. ix, cod. 68, pp. 102, 227. For good measure Manni added that Bartolomeo's death was dated in 1314 also by "Oudino," presumably Casimir Oudin (1638-1717), author of the *Supplementum de scriptoribus vel scriptis ecclesiasticis a Bellarmino omissis* (Paris, 1686) and of the *Commentarius de scriptoribus ecclesiae antiquis* (Leipzig, 1722). Neither of these works refers to Bartolomeo. Was Manni guilty of a little padding as well as of some plain old-fashioned lying?

131 *Chronicle,* p. 524. *Ammaestramenti,* ed. 1734, p. 31; ed. 1808, p. xxvii; ed. 1829, p. xxiv. Manni followed the *Memorie sacre* of Pietro Cardosi, who was responsible (p. 393) for the falsification of Bartolomeo's family affiliation; for similar activity on Cardosi's part in connection with Giordano, cf. n. 52, above.

led gullible Tiraboschi, who proceeded to enroll Bartolomeo in the "noble family of the Granchi."[132] Another widely consulted author fooled by Manni was a leading bibliographer.[133] A recent manual of Italian literature remarked about Bartolomeo: "Somebody erroneously said that he belonged to the Granchi family."[134] That somebody was Manni. Since he propagated a Redi-like falsification with regard to Bartolomeo's family, in the absence of proof shall we accept his assertion that Bartolomeo stopped writing the *Chronicle* in 1314?[135] The first group of disturbances in the chronological order of its entries would seem to associate the change of authors rather with the decade 1318-1328.

Apart from the three clusters of deviations, all four of these writers, it seems clear, intended to record the friars according to the year of their death.[136] But this information, as we saw above, is not given for Spina; and it is also lacking for the friar immediately before him, as well as for the one immediately after him. But the friar just preceding these three undated entries died in 1313 (the one before him died in December, 1312), while the friar who follows the three undated entries died in March, 1313. Hence we may with some assurance conclude that Spina died in the first quarter of the year 1313 (Roman style, since the entire *Chronicle* is based on that calendar).[137]

Did Giordano refer to Spina?

Giordano died in August, 1311 (*Chronicle*, p. 452). He had been summoned by the general of the Dominican order, Americo[138] of Piacenza, who was sending him to Paris to teach there. But death

132 Vol. 5, bk. 2, chap. 2, no. 37. So did Vincenzio Fineschi (*Memorie*, vol. 3, p. 110); D. L. del Zante, who wrote the account of Bartolomeo in *Biografia dei Pisani illustri* (Pisa, 1838); and Vincenzio Nannucci, who edited Bartolomeo's *Ammaestramenti* (Florence, 1840), p. 19.

133 Gamba, Bartolommeo. *Serie dei testi di lingua*. 4th ed., Venice, 1839, p. 11.

134 D'Ancona, Alessandro and Bacci, Orazio. *Manuale della letteratura italiana*. New ed., Florence, 1925-29, vol. 1, p. 264.

135 Manni's assertion was repeated by Fineschi (*Memorie*, vol. 3, p. 124); by Nannucci, in Bartolomeo's *Ammaestramenti* (Florence, 1840), p. 21; and by Galletti, p. 148.

136 Hence their joint product is a necrology, although called a chronicle (Masetti, vol. 1, p. 340; cf. n. 200, below). It does not "concern various instruments of antiquity," despite Rouyer, *Coup d'oeil*, p. 19. Those who wrote the necrology of S. Maria Novella referred to it as a chronicle (Orlandi, *Necrologio*, vol. 1, pp. 36.32, 74.8, 125.7, 262).

137 The error in Redi-Dati No. 2 ("Spina died in the year 1313 Pisan style, and 1312 Roman style") was falsely ascribed to the *Chronicle* by Del Guerra. *Ann. Ottalm.* 1933, *61*, 383. *Riv. Stor. crit. Sci. med. nat.* 1933, *24*, Atti, p. 17.

138 Misread as "Amico" by Redi (*Opere*, vol. 2, p. 261; vol. 5, p. 85), whose error was corrected by Manni (Manni-Biscioni, p. xxiv; *Biblioteca scelta*, vol. 384, p. xxix).

overtook Giordano at Piacenza,[139] about a hundred miles northwest of Pisa. His body was brought back to Pisa for burial in the church of St. Catherine. In the *Lettera* (*Opere*, vol. 2, pp. 261-262) Redi pointed out that Spina and Giordano both lived

> in the same monastery of St. Catherine of Pisa. Hence [Giordano] could with unquestionable certainty affirm what he said about eyeglasses in his aforementioned sermons. Likewise Friar Bartolomeo da San Concordio could truthfully write that Spina with his own mind discovered (ritrovò) the method of making eyeglasses and shared it with all those who wished to learn it, because this Friar Bartolomeo was a contemporary of Spina and lived with him in the same monastery of St. Catherine of Pisa.

Here Redi slyly summarized the *Chronicle* in such a way as to make it say: "Spina with his own mind discovered the method of making eyeglasses." If it is true that the *Chronicle* says (or implies) all this, it still is not the whole truth. For Redi craftily omitted from his summary of the *Chronicle* its statement that "eyeglasses were first made by someone else." In the eyes of the unwary reader, this omission left Spina alone in the field. Hence Redi could subtly imply that Giordano obtained his information exclusively from Spina: because the two friars lived in the same monastery,[140] Giordano "could with unquestionable certainty affirm what he said about eyeglasses."

We have finally uncovered the reason why Redi excluded from the *Lettera* this statement by Giordano:

> I have seen the man (colui) who first invented and created it [the art of making eyeglasses] and I have talked to him.

Of the two alternatives facing Redi, he chose suppression as the easier course to pursue at a time when Giordano's sermons were still all unpublished. For him the only other possibility was an attempt to show that by "colui" the preacher meant Spina.

Would Giordano have called Spina simply "colui"? In repeating the remarks of a dying prelate whose name he did not divulge, Giordano referred to him as "the cardinal of Ostia, who was a Dominican friar" (Narducci, *Prediche inedite*, pp. 356-357). On

139 Not at Pisa, despite the *Journal des sçavans*, 13 Feb. 1679; Tronci, Paolo. *Memorie istoriche della città di Pisa*. Livorno, 1682, p. 286; Molyneux, William. *Dioptrica nova*. London, 1692, p. 255; Smith, *Compleat system of opticks*, Remarks, p. 13; Kästner, Abraham Gotthelf. *Vollständiger Lehrbegriff der Optik*. Altenburg, 1755, p. 377; Caesemaecker, F. E. de. *Aenmerkingen over de brillen en derzelver gebruyk*. Ghent, 1841, p. 13; Hirsch, August. *Geschichte der Augenheilkunde*. Leipzig, 1877 (reprinted from Graefe-Saemisch, *Handbuch der gesammten Augenheilkunde*, vol. 7), p. 307; Fox, *Medical and surgical reporter*, 1890, 62, 514; Bock, *Die Brille und ihre Geschichte*, p. 10; De Waard, *Uitvinding der verrekijkers*, p. 63; and Reis, *Folia ophthal. orient.*, 1932-34, 1, 399; nor at Parma, despite Davidsohn, vol. 4, pt. 3, p. 70.

140 Despite Greeff (*Erfindung*, p. 74), the period of their co-residence does not include the year 1305, since Giordano was then in Florence.

another occasion (Manni-Biscioni, pp. 191-192) he spoke of the numerous sinners who came to him, and he asked, "Now how many are there who go to the other friars?" Once he permitted his listeners a precious glimpse at the psychology of monastic life (Moreni, vol. 2, p. 132):

> A friar of this order, who was forty years old when he told it to me, said that whenever he heard [the bell] toll for a meeting of the chapter, he was always very frightened, and went there with great fear, saying "I do not feel at fault, and yet each time I am so afraid that I hate to hear [the bell] toll for a chapter meeting."

When he was talking about fellow-members of his order, he called them "friars." But laymen of course received no such designation. A famous armorer was simply "a master [craftsman] of Naples" (Manni-Biscioni, p. 47). An auditor who opposed Giordano's preachment that "virginity is better than marriage" (Moreni, vol. 1, p. 214) by asking what would happen to the world if every man were a virgin, was nothing more than "someone" (uno).[141] Those who asked whether refraining from marriage was sinful were merely "some others" (alcuni altri; Narducci, *Prediche inedite,* pp. 118-119). Giordano's usage points to this conclusion: the inventor of eyeglasses was "colui," a nondescript, not a friar, and therefore not Spina, "whose habit and monkish status Giordano would not have refrained from mentioning" (Manni, p. 74). As Albertotti put it (1922, p. 86):

> For evident reasons it is difficult to understand why he [Giordano] . . . would have suppressed Friar Alessandro's name had the latter really been the inventor, or why he would not at least have mentioned the invention as an achievement of the Dominican order, had it been such.

Was Giordano perhaps disinclined to promote the prestige of his associates? On the contrary, he likened their activity to the apostles':

> Our moving about is for you, we circle through the city and go about preaching for you, to give you peace and let you share in it. Much more gladly would we stay in our cells and not go out, and we would have more quiet there, much more. *And Friar Giordano said,* I would gladly now stay in my cell for several years and not go out, and that would be very agreeable and very useful to me (Narducci, *Prediche inedite,* p. 250).

> If there were no preachers, *said Friar Giordano,* what would the world be? Worse than Saracens, or Tartars, or pagans, far worse. In the old days there used to be philosophers of great wisdom, and they repressed heresies and

141 Narducci, *Prediche inedite,* p. 117. An instructive contrast occurs in Giordano's reference to "one (uno) of the wisest friars who belonged to our order at that time." *Prediche sulla Genesi,* ed. Domenico Moreni. Florence, 1830, p. 95. Reprinted in *Biblioteca scelta di opere italiane antiche e moderne,* vol. 383. Milan, 1839, p. 107. Cited hereafter as "Genesi."

drove them out; but today it is not so. Who gives himself over today to wisdom, to learn, and to know wisdom? Scarcely do men know how to read, just scarcely; so that if there were no preachers, you wouldn't be able to describe the darknesses and the heresies that would exist.[142]

He extolled the members of his order as "holy friars who preserve their chastity and virginity" (Narducci, *Prediche inedite*, p. 194). But "everybody can't be so perfect. Well do we know that everybody can't be a monk or a hermit. We know that well" (Manni-Biscioni, p. 10).

There are many laymen who, having grown up amid vice, don't believe that others can get along without those vices. They marvel at the friars who can preserve their chastity, and they so marvel because to them it seems an impossibility. But it isn't. For from childhood on [the friars] are accustomed to the good and have practiced the virtues (*op. cit.*, p. 15).

But Giordano, after all, has heard that not all his colleagues are perfect (*op. cit.*, p. 9):

You ought to excuse the friars a little when they sometimes besmirch themselves, since being among the people and seeing the things of the world they can't help besmirching themselves.[143] They are men of flesh and blood like you, and of a vigorous age. It is rather a great marvel that they can stay so clean while remaining in the city and seeing the things of the world all day. For this city is no place for monks; rather the desert and solitude suit them. But we stay here among you, for our need, yes, but much more for yours. By ourselves we would get along much better, but how would you fare? You couldn't exist without us.

Such being his attitude toward laymen and monks, would Giordano have failed to proclaim the inventor of eyeglasses a friar had he been able to do so? In the words of Giovanni Batista Clemente de' Nelli (1725-93), biographer of Galileo and editor of his correspondence,[144]

When there are found enrolled in the various orders individuals outstanding in some profession or science, the monks do not overlook opportunities of publicly praising their superiority and of specially broadcasting their names for the greater glory of their persons; to omit this would be regarded by the entire group as a lack of respect for their noteworthy fellow-monk. Contrary examples can be adduced that when monks have occasion to mention some invention made by a layman, frequently they do not declare the inventor's name precisely, but indicate him in a general way. . . . [Had Giordano been thinking of Spina] he would have announced publicly that Spina, his fellow-Dominican and perhaps friend, was the one who first made eyeglasses, and all the more would he have praised that invention as the brain-child of an associate of his, whose name he certainly, in honor of the Dominican order, would not have omitted.

142 Moreni, vol. 1, p. 45; *Biblioteca scelta*, vol. 386, p. 155.

143 Since Giordano made so candid an admission in a sermon preached to the people of Florence, perhaps Boccaccio's picture of lechery among the clergy should not be condemned as overdrawn.

144 Nelli. *Vita e commercio letterario di Galileo Galilei*. Lausanne, 1793. pp. 161-162.

On Thursday, 4 November 1333, the river Arno, which flows between the two halves of the city of Florence, swept over its banks and inundated the town. The *Necrology* of S. Maria Novella, in its obituary of a certain friar, reports that[144.1]

> The Commune appointed him principal and sole superintendent of the entire task of rebuilding the Ponte alla Carraia, which had been destroyed by the flood. To his own honor and to the honor of the order, he completed this work in so praiseworthy a manner that thereafter he was continually and eagerly sought for other community projects.

Over and over again the *Necrology* praises friars for having added to the glory of their order.[144.2]

The author of the Spina entry in the *Chronicle* was also a member of the Dominican order. Would he have written that "eyeglasses were first made by someone else," had he had a fellow-friar in mind? The word translated as "someone else" is "alio"; in its place, as we saw above, Redi incorrectly read "aliquo," which Dati rendered in the *Veglia* by "un tale" (Targioni, p. 60). For the *Chronicle's* "alio," the *Annals* substituted "quidam" (a certain person). But "quidam" is not permitted to stand by itself when the *Chronicle* refers (p. 523) to a friar: "audivi a quodam fide digno Fratre antiquo" (I have heard from a certain trustworthy old friar). The *Chronicle's* usage matches Giordano's. Friars are specifically so designated. The inventor, not being so designated, was therefore a layman. Otherwise, as Albertotti asks (1922, p. 86), "Why also here all that reticent modesty in concealing the truth about a fact which would have redounded to the glory of the Order?" Giordano's "colui,"[145] the *Chronicle's* "alio," the *Annals'* "quidam," Dati's "un tale,"[146] was not a friar, and certainly not Spina, despite Redi, Tempesti (p. 61), Bonaini (*Chronicle*, p. 478), Del Lungo (pp. 7-8), and Lucio Spera.[147]

(To be concluded)

144.1 Orlandi, *Necrologio*, vol. 1, p. 52.22-29; cf. vol. 2, p. 426, no. XIII.

144.2 *Op. cit.*, vol. 1, pp. 103.22-24, 114.20-22, 154.3, 169.4-5, 195.1.

145 Bourgeois (*Trav. Acad. nat. Reims*, 1903, *113*, 159; *Besicles*, p. 11; *Aesculape*, 1923, *13*, 146) would have us believe that Giordano saw the inventor and talked to him without knowing his name.

146 Merged with Spina by Greeff, who failed to understand the Italian text he undertook to translate (*Z. ophthal. Opt.*, 1917, *5*, 75; *Erfindung*, p. 77).

147 Chi ha inventato gli occhiali? *Il giornale d'Italia*, 8 Sept. 1920, p. 3.

Jacques Loeb and the Method of Double Stimulation

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Sensory Extinction

THE phenomenon of extinction of sensory responsiveness as a consequence of simultaneous double stimulation, which is observed in certain patients with cerebral disease, is currently a topic of considerable interest in both clinical neurology and physiological psychology. Extinction in the visual and tactual spheres has been extensively studied [1, 7] and the deficit has been described as occurring in other modalities, such as proprioception,[4] taste,[2] and audition.

Visual extinction as a function of simultaneous double stimulation may be described in outline as follows: a single stimulus applied to either the right or left visual field is perceived by the patient. When, however, stimuli are applied to both fields, the patient reports seeing an object in only one of the fields. A favored method of testing for the deficit is to stimulate the suspected field, secure a report from the patient that he perceives the stimulus, and, then, while continuing the initial stimulation, to apply a second stimulus to the healthy field, noting whether there is any change in responsiveness to the first stimulus.

The deficit occurs in varying degrees of severity. There may be complete cessation of responsiveness to the affected field with double stimulation ("extinction"). However, under this condition, the patient may still perceive the stimulus but report only that it has become somewhat dimmer or darker ("obscuration"). Sometimes the deficit cannot be elicited with standard testing procedures but becomes manifest if the patient is required to report or reproduce from memory what he has seen.[5] In other cases, significant augmentation of the strength of the stimulus in the affected visual field enables the patient to perceive both stimuli. Since the patient often behaves as if he were "neglecting" the stimulus in the affected field, the term, "hemianopic weakness of attention" was coined by Poppelreuter[17] as a name for this type of impairment. Thiébaut and Guillaumat[19] called the deficit "relative

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hemianopsia." Finally, on the basis of the idea that the phenomenon may involve activity of assumed "suppressor systems" in the cortex, the designation of "sensory suppression" has been employed by some writers.[8, 18]

Tactual extinction occurs in analogous fashion. A touch or pain stimulus applied, for example, to either shoulder, forearm, or palm is perceived by the patient, while double stimulation results in his reporting that he feels only the stimulus on one side.*

The clinical significance of the extinction phenomenon and the method of double stimulation lies in the fact that they disclose behavioral deficits which are not elicited by other methods of examination. In turn, these deficits point to dysfunction in the central nervous system and, in conjunction with other signs, are often of value in localizing the dysfunction. Thus, many of Bender's patients, who showed no visual field defects by routine perimetry, manifested behavioral impairment in the double stimulation test—impairment which disclosed the presence of central nervous system disease and which was helpful sometimes in the localization of the underlying lesions.

In addition, the phenomenon and the method possess broader psycho-physiological implications. As Bender and Teuber[3] have pointed out, it seems likely that the clinical findings represent a special case of a general principle of psychological functioning, *viz.*, that in the interaction of simultaneously occurring stimuli or response tendencies, the stronger tendency will inhibit the weaker. In the intact subject, a marked difference in the strength of the two stimuli is usually required to secure overt behavioral expression of the process. In the patient with nervous disease, the strength of the two response tendencies is differentially determined by intra-organismic factors so that the phenomenon may occur within the setting of double stimulation of objectively equal strength.

HERMANN OPPENHEIM

The method of simultaneous double stimulation was introduced into clinical neurology in 1885[14] by Oppenheim who wrote a paper on its possible clinical significance and subsequently made mention of it in the various editions of his well-known textbook of

* These remarks actually cover only one form of extinction, that of the homologous crossed type. There are several other types (e.g., ipsilateral, non-homologous crossed, interaction across sensory modalities) which are described in the monographs of Bender and Critchley.

neurology.[15] Thus, Oppenheim is generally credited with being the discoverer of the method, at least insofar as its neuropathological application is concerned.*

In the second edition (1898) of the textbook, Oppenheim described the tactual stimulation procedure as follows:

> In certain cerebral diseases, which lead to unilateral sensory disturbances, I have often employed the following method of examination: two symmetrical locations on both sides of the body are simultaneously stimulated with touching by a brush or by pin-pricks; under these conditions it happens that the patient invariably perceives only the stimulus which has been applied to the healthy side, while with single stimulation he feels every stimulus on the affected side. We shall designate this mode of examination as the method of double stimulation. (p. 51)

Following deliberate policy,† Oppenheim did not include references to the literature in the earlier editions of his textbook, and there is no mention of previous work on the method of double stimulation, not even his own paper in the *Centralblatt*. However, beginning with the fifth edition (1908), he did add—with apparent reluctance—selected references.‡ In this edition, in the paragraph describing tactual extinction which is quoted above, reference is made to the *Centralblatt* paper. The seventh edition (1923) of the textbook, appearing after Oppenheim's death and revised by a number of his students, includes an additional reference with respect to the method of double stimulation: "At Oppenheim's suggestion, Medea (Atti della Soc. Milano, 1908) has studied this problem in detail."§

Thus, in the various editions of the famous textbook, there is no suggestion that Oppenheim might not have been the originator of the method of double stimulation and its application to problems of cerebral dysfunction. However, in his original paper in the *Centralblatt*, Oppenheim indicates clearly the direct source of the method. The paper begins as follows:

> By means of superficial, circumscribed injury of the brain in dogs, Jacques Loeb elicited disturbances in function, the nature and interpreta-

* One exception to this rule (the only exception of which the writer is aware) deserves mention. In a recent paper, Krueger, Price, and Teuber,[9] citing Jacques Loeb's 1884 paper (*vide infra*), point out that he was the first to use the method of double stimulation in animal experimentation.

† See the preface to the first edition (1894).

‡ "The principal change which occurs in this new edition of my textbook is that, yielding to the pressures of friends of the work, I have decided, after much doubt and long hesitation, to cite references to the literature." (From the preface to the fifth edition.)

§ The reference is to Medea (13).

tion of which he has reported in a brief communication in Volume XVII of Pflüger's Archiv. This author has had the kindness to demonstrate repeatedly the interesting phenomena in his experimental animals to me and thereby has stimulated me to make the investigations, the results of which I now briefly report.

Oppenheim then described four patients with focal lesions. Two of these showed tactual extinction on double stimulation, one showed both tactual and visual extinction and the fourth showed tactual and auditory extinction. He pointed out that the phenomenon never occurs in healthy subjects and, in fact, is found to be relatively infrequent in patients with cerebral disease. He also observed that augmentation of the strength of the stimulus to the affected side could lead to perception of both stimuli by the patient.

THE CONTRIBUTION OF JACQUES LOEB

Oppenheim's reference to "Volume XVII" of *Pfluger's Archiv* is evidently a typographical error. The paper which he almost certainly had in mind is in Volume 37 (1885) and bears the title, "Die elementaren Störungen einfacher Functionen nach oberflächlicher, umschriebener Verletzung des Grosshirns."* This paper was written on the basis of work which Loeb had done in the laboratory of Goltz in Strassburg.[16] In it he describes the method of simultaneous double stimulation with particular reference to the visual modality, reports his findings with the method on intact and operated dogs, and clearly indicates the neurophysiological significance of the extinction phenomenon, i.e., the implication of dysfunction in the contralateral half of the central nervous system. He begins the paper by stating two "laws of elementary disturbances of function."

If we designate the side of the body and visual half-field which corresponds to the injured hemisphere as "same-sided" or the "side of operation" and the other as the "crossed" or "contralateral" side, the two laws of elementary disturbances of function read as follows:

I. If two stimuli, which are equal to each other in quality and intensity, are applied simultaneously to symmetrical parts of the retinae or skin, the stimulus applied to the crossed side is less effective than the one which is applied to the side of operation. However, by augmentation of the intensity of the stimulus to the contralateral side the same effectiveness can be achieved.

* In passing, it should be noted that in a comprehensive paper on visual and psychic disturbances following cerebral injury in the dog, which appeared a year earlier,[10] Loeb briefly described his findings with double stimulation. However, he reserved detailed discussion of its significance for the 1885 paper.

II. When a stimulus of a given quality and intensity is applied to the crossed side, the reaction has a longer latency and is slower than when the same stimulus is applied to the side of operation.

Three basic observations, which "can always and without exception be demonstrated, with constancy of a physical experiment," are then cited.

(1) If one suddenly and simultaneously presents pieces of meat in two symmetrical parts of the visual field of the hemi-amblyopic dog, the dog consistently takes the one situated on the side of the operation.—(2) If one lightly oscillates the meat situated on the contralateral side—a moving object is a stronger stimulus—, while the other piece of meat remains stationary, the animal will spring at the moving piece of meat. The rate of oscillatory movement which is necessary for this effect varies with different animals. Within certain limits, the more severe the hemi-amblyopia the higher the rate must be.—(3) If a piece of meat is presented alone in the crossed half-field to the dog, he regularly springs at it, even when it is not moved. Thus, while this stimulus, when it is presented alone, is adequate to elicit a reaction on the part of the dog, it remains always without effect if at the same time a second stimulus, of the same quality and intensity, is applied to the side of operation.

In a third paper,[12] which appeared a year later, Loeb again described the method of double stimulation as a means of disclosing half-field defects. He points out that the most serious immediate effect of this character after operative injury to a cerebral hemisphere is complete lack of responsiveness in the contralateral visual half-field, even to very strong stimulation. There is then gradual postoperative improvement so that after a few days oscillating objects or objects which are quickly introduced into the affected half-field are reacted to, while there is still no responsiveness to objects brought more slowly into the field. Progressive improvement continues beyond this intermediate stage.

The intensity of stimulus in the crossed visual half-field, which is necessary to elicit a reaction on the part of the dog, decreases steadily. After a time, if a single piece of meat is presented to the dog, he will react to it, even if it is not moved. In addition, when the meat is moved or thrown to the right, the dog follows it just as he would if it were moved or thrown to the left. It would appear from these tests that a visual disturbance is no longer present. However, if two pieces of meat are suddenly presented simultaneously to the dog, one in the right and the other in the left visual field, the animal who has been operated in the left hemisphere will without exception take the piece of meat to the left.

These findings record the course of a rather severe hemi-amblyopia from the time of operation to the restitution of visual capacity; however, after superficial lesions and favorable conditions of operation and recovery,

the initial disturbance is not as severe as here described. In the majority of cases, only the last described stage is present from the very beginning: the dog takes single pieces of meat under all conditions and favors that situated on the side of the lesion only upon simultaneous presentation of two pieces of meat.

Specific observations on the animals' reactions to simultaneous double tactual stimulation are not recorded although references to it are made in the first "law" and in the statement that one-sided disturbances of sensibility, analogous to the visual disturbances, occur. Loeb's explanation of the extinction phenomenon was that injury to a hemisphere resulted in increased resistance to conductivity in the involved nervous pathways, with a consequent weakness and retardation in the behavioral reactions mediated by these pathways. He felt that his two "laws" had broad applicability to all central nervous system function, spinal as well as cerebral.

It seems evident that Loeb had a clear understanding of the significance of the method of simultaneous double stimulation. He described sensory extinction accurately and pointed out the importance of the factor of relative strength of stimulus in determining whether extinction would occur or not. He was obviously aware of the lateralizing neurophysiological implications of the phenomenon. One misses in his exposition any explicit indication that the extinction effect is a resultant of the interaction or "competition" of simultaneous stimuli, with the stronger stimulus in some way suppressing or inhibiting the effectiveness of the weaker one. However, this is equally true of Oppenheim's treatment, which is restricted to the practical clinical significance of the extinction phenomenon.

A year after the publication of Oppenheim's paper in the *Centralblatt,* Bruns, an assistant at Hitzig's clinic in Halle, published a confirmatory case report[6] of a patient who showed tactual extinction on double stimulation and credited Oppenheim with having "discovered a hitherto unrecognized type of sensory disturbance by means of a newly applied method of clinical examination." Loeb was not mentioned despite Oppenheim's frank acknowledgment of his work. This is perhaps understandable in view of the purely clinical import of Bruns' brief paper. However, the failure to mention Loeb may also have been conditioned by the extremely acrimonious controversy on cerebral function between Goltz (and his able lieutenant, Loeb) and Hitzig (who was Bruns' chief), which was then going on.

In any event, Loeb's pioneer contribution was seemingly forgotten as the years passed and is only now once again being recognized.

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Charles Badham, M. D., F. R. S. (1780-1845)

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THE Regius Chair of Medicine in the University of Glasgow is rightly regarded as one of the most important posts in Scottish medicine. It is therefore of interest that since the Chair was instituted in 1637 only two of the seventeen occupants have been Englishmen. One of these, Professor E. J. Wayne, is the present holder; the other was Charles Badham who was professor from 1827 to 1841. In spite of an excellent paper by Finlayson (1900), little attention seems to have been paid to Badham who was a notable character in his day. The following information about his life and accomplishments may therefore be of interest.

Charles Badham was born in London on 17 April 1780 and is said to have received a good classical education at Westminster School before proceeding to the study of medicine. He graduated M.D. at Edinburgh in 1802 with a doctoral thesis entitled "De urina et calculis urinariis." It is written in Latin, as was the custom, and is dedicated to his father, David Badham, and to Charles Smith. An elementary treatise, it contains no remarkable statements. After Badham qualified in medicine at the age of 22 his life can suitably be divided into three periods.

The *first period* from 1803 to 1810 was occupied principally by practice and teaching in London. It was during this early period that he staked his claim to a place in the history of medicine. In April 1803, Badham became a Licentiate of the College of Physicians of London. This was necessary because at that time Scottish medical degrees were not accepted in the capital. Immediately thereafter he was appointed a physician to the Westminster General Dispensary and began to teach. The *Medical and Physical Journal* for October 1803 contains the following advertisement: "Dr. Badham one of the Physicians to the Westminster Dispensary, Gerard Street, proposes to deliver during the winter two courses of lectures on Chemistry." At first Badham taught in association with Alexander Crighton (1763-1856), but in 1804 Crighton went to Russia as physician to Tsar Alexander I, and thereafter Badham continued teaching on his own. Advertisements of lectures by Bad-

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ham on Chemistry, Medicine, and Materia Medica continued to appear until 1807. His lectures were given at the Medical Theatre, Clifford Street, Burlington Gardens and were combined with two clinical lectures weekly at the Westminster Dispensary.

In 1805 Badham published in the newly established *Edinburgh Medical and Surgical Journal* a paper entitled: "Practical Observations on the Pneumonic Diseases of the Poor," based on his experiences at the Dispensary. This paper makes interesting reading. He begins by discussing "pleuritis" and stresses the insidious nature of this disease even in cases which end in suppuration. He strongly advocates bleeding as the most valuable therapeutic measure, especially in children. "In the beginning of this winter, from a child of two years I took ten ounces of blood within 48 hours. The child was bled thrice and would not I believe have recovered without the third bleeding." He indicates his preference for venesection rather than leeches which are "somewhat uncertain and besides scarcely within the purchase of the poor." There follows a description of a condition which he calls "pleuritis rheumatica" and which he regards as a form of rheumatism affecting all the organs of respiration. It is tempting to think that this may be an early description of Bornholm disease, but I think it is unlikely. The final section of this paper deals with hydrothorax. It is easy to recognise from his excellent clinical descriptions that he was dealing with cases of cardiac failure with fluid in the chest. Although he does not recognise the cardiac origin of the condition, he mentions that a combination of digitalis and mercury is the most effective treatment.

Three years later (1808) Badham published his major contribution to medical literature. By this time he was physician to H.R.H. The Duke of Sussex and to him Badham dedicated his monograph *Observations of the Inflammatory Affections of the Mucous Membrane of the Bronchiae.* In this little volume Badham used the term "bronchitis" for the first time and separated bronchitis as a disease entity from other diseases of the chest. The Oxford English Dictionary (1933) attributes the introduction of the term bronchitis to P. Frank (*Interpretationes Clinicae,* 1812) and to Bodham (*sic*) (*Inflammatory Affections of the Bronchia,* 1814), but there is no doubt that Badham was using the word freely in 1808. He describes three forms of bronchitis: bronchitis acuta, bronchitis asthenica, and bronchitis chronica. His bronchitis asthenica he identifies with the "peripneumonitis notha" of Sydenham and his bronchitis chronica with the "chronic cough," "tussis

cum dyspnoea," "catarrhus pituitosus," and "catarrhus senilis" of previous authors. His recognition of acute bronchitis as a disease entity different from pneumonia and pleurisy is the most important part of the book. He describes the sudden onset with constriction in the chest, severe dyspnoea, paroxysmal cough, at first without expectoration, but later productive of large amounts of yellow sputum. He gives several case histories. Some of these would now be regarded as cases of bronchiectasis or bronchopneumonia, but on the whole he does present an accurate description of severe acute bronchitis as we recognise it today. He considers that the purulent material which fills the bronchi and which is expectorated in the later stages of the disease is a secretion of the bronchial mucous membrane and sets out valid reasons why he has reached this conclusion. Acute bronchitis may be diagnosed from peripneumonia and pleurisy by the absence of a sharp pain in the side and the presence of a general discomfort in the chest, by the fact that the patient can turn without increasing his pain, by the presence of wheezing in some cases, and by the pulse which, although frequent and hard in both diseases, in acute bronchitis does not have the "vibrating quality" observed in pneumonia. For the treatment of the condition he advocates bloodletting as the "great remedy" but states that it is not so efficacious as in other pulmonary complaints. He is not enthusiastic about the value of blisters, purgatives, or sudorifics. He discusses at length the value of the various expectorants—ipecacuanha, squill, antimony, etc.—and concludes that they are of little proven value. "The treatment of this complaint is a good deal committed to what are called expectorant remedies. After prescribing these drugs, in many forms and various combination, for a good many years and in some hundred cases, the author cannot say he thinks highly of their efficacy—and yet some efficacy they have: for it certainly consists with observation, that one shall fail, and another, yet under the use of a third, a patient shall be very sure that he finds relief; although it is true, that by the time he makes trial of the third, a natural period of relief may have arrived; still we must allow something to medicine."[1]

Opium should be used with reserve and only when the febrile stage of the illness is past. His other sovereign remedy for acute bronchitis was calomel which he administered to one patient, in a dosage of 1 grain every two hours up to a total of 18 grains, by which time "the mouth was rather tender, and it was discontinued"!

[1] The last phrase is omitted from the second edition.

For the treatment of chronic bronchitis he advises rubbing the chest with a liniment and he states that the patient should be confined to the house during the winter months so as to maintain a uniform environmental temperature. Opium should be used sparingly and only at night as cough cannot be completely suppressed. He again belittles the value of expectorants but suggests that the breathing of "factitious airs," a remedy of which he had no personal experience, might help the chronic bronchitic.

This book was well received. The lengthy reviews in the *Edinburgh Medical and Surgical Journal*[2] and in the *Medical and Physical Journal*[3] are on the whole complimentary which is more than can be said of most reviews of that period. A second edition with the title changed to *An Essay on Bronchitis,* etc. was published in 1814. This edition was translated into German.

The *second period* covers the years 1810 to 1827. Little information is available about Badham's activities during these years, but there is evidence that his interests changed and that he became more interested in literature, the classics, and in travel than in medicine. In 1806 he had matriculated as a gentleman commoner at Pembroke College, Oxford, and from there he obtained third class honours in Litterae Humanioribus in 1809, graduated B.A. in 1811, M.A. in 1812, M.B. and M.D. in 1817. In 1812 appeared his *Specimens of a New Translation of Juvenal.* These were severely criticised by the editor of *The Quarterly Review,*[4] Dr. Gifford, who was himself the author of a translation of Juvenal. Undeterred by Gifford's slashing criticism Badham published his full translation, *The Satires of Juvenal,* in 1814 and this, too, was severely dealt with in *The Quarterly Review.*[5] He also traveled widely. Munk (1878) states that he spent half his days in travel and therefore did not have as large a medical practice as his talents would have entitled him. Prior to 1815 the Napoleonic Wars prevented him from indulging in Continental travel, but after the conclusion of hostilities he set off on the Grand Tour of Europe and spent two years mostly in the countries bordering on the Mediterranean; Italy, the Ionian Islands, Albania, where he was consulted by Ali Pasha, and Greece.

For the next ten years (1817-1827) Badham appears to have done little. It is probable that he spent most of this time as a private

2 1808, *4,* 493-500.
3 1808, *20,* 78-81.
4 1812, *8,* 60-65.
5 1814, *11,* 377-398.

travelling physician to various lords and ladies. It is said that during this period of his life he went through a private fortune of £16,000 to £20,000![6] In 1818 he was elected a Fellow of the Royal College of Physicians "by right and not by favour" and in the same year he was made a Fellow of the Royal Society.

The *third period* was that during which he occupied the Glasgow chair. In 1827 Robert Freer died at the age of eighty years. He had held the Regius Professorship of Medicine at Glasgow for thirty-one years. For two years before his death he had been unable to lecture and his nephew Dr. Whitson had carried out his duties. Whitson, who was a preacher, was in the habit of reading his uncle's lecture notes verbatim, and one of his contemporaries, writing in the *Lancet*,[7] said of him that he "never will make even an ordinary lecturer." In addition to Whitson there was another strong local candidate for the Chair: Richard Millar who for thirty years had been lecturer in Materia Medica. Millar was a good teacher and popular with his students and colleagues. His promotion to professorial rank was consistently refused because of his political views.[8]

The appointment nominally made by the crown was really in the hands of the Chancellor of the University, the Duke of Montrose. At this time the Duke was an invalid and his attending physician was Sir Henry Halford, then President of the Royal College of Physicians. It was natural that the Chancellor should consult such an eminent physician on the subject of the appointment of a Professor of Medicine, and Halford recommended Badham for the post. Whether he did so because of Badham's literary and medical talents or "to get rid of a nightmare, the pauper of the College of Physicians," as a contemporary correspondent[9] to the *Lancet* suggested, we have no means of judging.

Badham's appointment caused consternation in Glasgow. He was "wholly unknown to the medical and literary rulers of this western emporium of rum, cotton and learning."[10] Moreover, it mortally offended the pride of the local medical profession to see an Englishman nominated to this important Scottish chair. The *Lancet* for 1827 contains several letters condemning the appointment on the grounds that it was wholly unsuitable for one versed

6 "Amicus Justitiae," *Lancet*, 1827, *12*, 463.

7 "Glasguensis," *Lancet*, 1827, *12*, 248.

8 In 1831 Millar became the first Professor of Materia Medica at the University. This appointment coincided with the return to power of Sir Robert Peel's Tory Government.

9 "Amicus Justitiae," *loc. cit.*

10 "Glasguensis," *Lancet*, 1827, *12*, 343.

in English practice to teach Scottish students. In these letters Badham is described as a travelling physician with lords and ladies who is "pedantic, wayward and intemperate."[11] Only one letter, signed with the initials "J.C.B.,"[12] welcomed the appointment and described the new professor as a "scientific physician, elegant scholar and a man of very extensive literary attainments."

From the first Badham and the University authorities were at variance. According to Coutts (1909) he came to be admitted in June but had to wait until October before a quorum was obtained. He then quarrelled with the University over the question of graduation fees. It had been the practice for these fees to be divided equally between the Professor of Medicine and the Professor of Anatomy but it was now proposed that they should be divided among all the teachers in the Medical Faculty. Badham, whose funds were at a low ebb, objected to this interference with his perquisites and refused to examine. Before doing so he secured, in 1828, for his son John Badham the degree of M.D. *in absentia* and without personal examination "as severe illness renders it necessary for him to leave this country for a warmer climate without delay."[13]

Badham was the first holder of the chair to be styled Professor of the Practice of Medicine. When he was appointed, the Crown reserved the right to nominate a second professor to deal with the "Theory" or "Institutes" of Medicine (physiology, pathology, preventive medicine). This intention was not put into practice until 1839. At first Badham himself lectured on both "Theory" and "Practice," then for two years the lectures on the "Theory of Medicine" were given by his son David Badham. In 1832 David Badham was appointed to the Radcliffe Fellowship of Oxford University, and thereafter Dr. Harry Rainy was appointed lecturer on the Theory of Medicine.

Badham's first course of lectures was well received. The *London Medical Gazette* of 10 May 1828 contains a "Letter to Dr. Badham from his pupils at the conclusion of his first course of lectures," in which they offer him sincere congratulations on the successful issue of the course and express the hope that he will long continue to occupy the chair so that "the benefit of a talent so

11 "Amicus Justitiae," *loc. cit.*

12 J.C.B., *Lancet*, 1827, *12*, 350. The initials are possibly those of Dr. J. C. Balmanno, Physician to the Royal Infirmary and twice President of the Faculty of Physicians and Surgeons.

13 Addison, W. I. *A roll of the graduates of the University of Glasgow 1727 to 1897*. Glasgow, James MacLehose & Sons, 1898, p. 26.

diversified, erudition so extensive and taste so refined may continue to be duly estimated and gratefully enjoyed by the students."[14] This enthusiasm seems to have been short-lived for in 1833 a correspondent in the *Lancet* complained that his lectures "convey no practical knowledge but are more poetical and literary than on medical subjects."[15] Duncan (1896) says, "His prelections were academic and bore little relation to practice and were more related to belles lettres, etc." It is also reported that he spent two to three weeks on poetical criticism and in a description of his travels and hair-breadth escapes on the Continent. Munk (1878), however, says that "Dr. Badham's lectures displayed so much ability that his colleagues soon discovered that they had reason to be proud of the services of so distinguished a professor."

Fortunately we are not entirely dependent on these conflicting opinions for our estimate of the value of Badham's teaching because portions of some of his lectures were published in the *London Medical Gazette*. These make heavy reading today. The lectures are full of pedantic, flowery phraseology and lengthy sentences constructed in the classical style. Every opportunity is taken to bring in allusions to classical literature, and he devotes much time to the praise of ancient medicine as practised by Greek and Roman physicians. Some idea of his style may be gathered from the following passage in reference to bloodletting: "after those depletions of the sanguiferous system shall have been performed which prudence will adjust to the severity of the case. . . ." In an address delivered on the occasion of the annual distribution of prizes in 1829 he discusses the value of medical education as a discipline for its own sake: "I may venture to claim for the student of medicine that he be at least equally welcomed, encouraged and applauded with the fabricator of faultless pentameters and immaculate iambics." In another lecture he advocates that medical students should be examined in the wards and not only by oral or written examinations as was the practice at that time. He also makes the suggestion that, as in ancient times, the general public should be admitted to witness these examinations! From the published samples of his lectures it is difficult to obtain any idea of Badham's views on the diseases of his time, but there is no indication that these were in any way unorthodox. As might be expected he was inclined to be conservative. At this time the new science of pathology was receiv-

[14] *London Medical Gazette*, 1828, *1*, 712.

[15] Letter to Viscount Melbourne by a member of the University of Glasgow. *Lancet*, 1833, *1*, 635.

ing much attention especially on the Continent. Badham expressed the opinion that pathology had not helped to clarify the nosology of disease and that the pathologists were as full of hypotheses as the old physicians like Hippocrates and Sydenham. He was not a great believer in the efficacy of drugs and mistrusted polypharmacy. He advocated bleeding for many diseases but warned against its excessive use and as an example quoted a case in which the removal of 60 ounces from a pregnant woman was followed by disastrous results.

There seems to be no doubt that after the first few years of his tenure of the chair Badham's lectures were unpopular with the students and, as he lectured only twice or thrice a week, his class tickets were not accepted for degrees in Edinburgh.[16] On the other hand, Dr. Rainy's lectures on the Theory of Medicine were exceedingly popular and his class numbered more than 140 students. This was greatly to Badham's advantage as he received one-third of the fees from this class. When in 1839 the Chair of the Institutes of Medicine was established, Rainy was passed over by the Crown, probably on political grounds, in favour of Dr. Andrew Buchanan. Badham was not in Glasgow at this time but he wrote from Brighton to the *London Medical Gazette* protesting strongly against the decision to appoint Buchanan.

Badham was frequently absent from the University. In October 1833 he was granted leave of absence for two months on account of the illness of his son. In October 1839 he wrote from Paris asking again for leave of absence and requesting that a substitute should be appointed to give his lectures. The University authorities, thoroughly disgruntled at their Professor of the Practice of Medicine, replied that he must produce a medical certificate and informed him that his substitute should receive *all* the students' fees. Medical certificates were duly produced; Rainy was then nominated as Badham's substitute and lectured in 1839 and again the following year (Coutts, 1909).

Badham made no attempt to practise medicine in Glasgow; "an erudite lecturer in physic, the learned professor was well known to eschew the practice of it" (Duncan, 1896). He was ex-officio a Director of the Royal Infirmary but it was not until 1830 that the managers in a dire extremity because of an acute shortage of "pure physicians" in the city elected him as assistant physician. Badham accepted this appointment on the understanding that "it

[16] Letter to Viscount Melbourne, etc., *loc. cit.*

would never be required of him during his clinical course to attend fever cases" (Buchanan, 1832). In one of his lectures Badham himself says that he discharged his duties at the Infirmary for only three months. His name is not on the roll of the Faculty of Physicians and Surgeons and he took no part in the social life of the city. As soon as the academic session was over he was off to the south of England or to the Continent.

If Badham's appointment had caused consternation in the University and in the city, his resignation came as a bombshell. On September 1841, without prior warning, the Principal received from the Home Secretary notification of Badham's resignation and a commission appointing as his successor William Thomson, brother of the better known Allen Thomson, then Professor of the Institutes of Medicine in Edinburgh but later Professor of Anatomy in Glasgow. This commission contained the provision that Thomson was to pay Badham £300 for life. (The stipend of the chair at this time was £270 of which the University paid £220 and the Crown £50; to this were added the students' class and examination fees.) This provision which seems extraordinary was quite legal. There was no pension or superannuation scheme for retired professors and this was the method adopted to secure a retirement allowance for them. Soon afterwards, this highly unsatisfactory arrangement was abolished (Finlayson, 1900). Before he retired Badham had for two years tried unsuccessfully to convince both the University and the Government that it would be a good idea if the method of patronage were adopted for University chairs, a professor being allowed to nominate his successor. His intention was to resign in favour of his son so that the University would get "a well educated, travelled young man, instead of a peevish old invalid." The University authorities had had quite enough of the Badham family and neither they nor the Home Secretary (Lord John Russell) thought highly of this proposal for dynastic succession!

During the Glasgow period Badham continued his literary activities. In 1827, the year of his appointment, he had printed privately a poem "Lines written at Warwick Castle." Two years later this poem was published in *Blackwood's Magazine*. In 1831 a new edition of his translation of the Satires of Juvenal was published in Valpy's Classical Library. This was dedicated to Sir Henry Halford, P.R.C.P., who had secured for him the Glasgow appointment, and contained an appendix with "imitations of the

third and tenth satires by Dr. Samuel Johnson." In 1835 he had printed privately a book of poems entitled *Brief Recollections chiefly of Italy, by an Amateur.* This slim volume was published anonymously, but the identity of the author was no secret. Badham dedicated it to the headmaster of Eton in return for advice given by the latter about the education of his sons. It contains ten poems including yet again "Lines written at Warwick Castle." Only one of the poems has any relation to the medical profession and is about "The Gold-Headed Cane," the famous staff which had recently been presented to the College of Physicians. In a note which follows this poem Badham deplores the lack of classical education among medical students at the Scottish universities but condescends to say "in Glasgow it is true that I have known several whom I am ready to *presume* good scholars." In 1840 he was Harveian orator of the Royal College of Physicians but his address was not published and its title and contents are not recorded (Munk, 1878). His last medical publication in 1841 was a letter to the *London Medical Gazette* from Naples describing a case of perforation of the stomach in a young girl and recalling a similar case which had been under the care of his son.

After his retirement from the Glasgow chair, Badham lived for four years. He died in London on 9[17] November 1845 and was buried in St. George's, Hanover Square.

Looking back from the vantage point of more than a century how are we to assess Badham's worth? Finlayson (1909) dismissed him as "an interesting figure which flitted across the medical world of Glasgow sixty or seventy years ago: a professor of Medicine in the University of Glasgow with about as little real relationship to medicine as, personally, he had with the life of Glasgow." This may be a true assessment of Badham's fourteen years in Glasgow but I think he deserves more serious consideration. There is no doubt that in his early years he showed signs of becoming a first-class practical physician. His early paper on the "Pneumonic Diseases" and his monograph on bronchitis, both written before he was thirty, are in sharp contrast to his later writings. They are in straight-forward, simple language and show evidence of clinical acumen and original observation. At this time he must have been very interested in clinical medicine and well read in the subject, for his 1805 paper contains a reference to Auenbrugger's work on percussion. Auenbrugger's monograph was published in 1761, but

[17] The date of his death is erroneously given as the 10th in the *Dictionary of National Biography.*

it was not until 1808 that Corvisart's French translation made his method generally known.

There can be little doubt that a radical change in Badham's outlook and in his attitude to medicine must have occurred between the years 1810 to 1815. From that time onward his approach to medicine was subordinate to his interest in the classics, and there is no evidence that thereafter he practised medicine except for his attendance upon the Duke of Sussex and other travelling lords and ladies. Certainly sometime between 1808 and 1814 he gave up or was displaced from his appointment to the Westminster General Dispensary. In Glasgow, apart from his three months' attendance at the Royal Infirmary, his relationship with medicine seems to have been entirely academic. How strange that he should have been the first Professor of the *Practice of Medicine!* Reading the available samples of his lectures one can imagine that they must have been very unsuitable for undergraduates anxious to learn to become practical physicians and surgeons.

His relationship with the University was obviously unhappy. Now, one can see that there were faults on both sides. His appointment was resented and even before he arrived doubts were being cast on his suitability for the post. The University appears to have made little effort to welcome him; instead they chose the time of his arrival to institute reforms which were bound to be resented by a new Professor and especially one of Badham's temperament. Badham, on the other hand, regarded his professorship as something of a sinecure and reckoned that if he gave his annual course of lectures, he had fulfilled his duties. I doubt whether he ever attempted to settle or to practise medicine in Glasgow but I think the way of any incomer from England who had attempted to do so at that time would have been very hard.

Although his classical knowledge must have been considerable, he does not seem to have gained much distinction as a classical scholar; in this respect he is overshadowed by his son. Today his poetry is quite forgotten. If he is remembered at all, it is as the physician who first used the term "bronchitis" to describe what is now one of the commonest respiratory diseases.

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The G. Palmer Story

(Or, *What It's Like, Sometimes, To Be A Scientist*)

GORDON L. WALLS*

IT is over three years now since I picked up the last little clue, and added to all the rest it still was not enough. I do not suppose now that I shall ever know exactly who he was or what his roots were. His real name may have been George Palmer or at least G. Palmer; or, Giros von Gentilly, Girod de Chantilly, or something-or-other Girod—just possibly even Amury Girod. I think that I have found out as much about him as any one person could, but I know I went at it in a most bumblesome fashion. Let others be warned by my tale not to attempt historical research without any particular training for it.

It all started from the fact that my chosen field of scientific investigation is color blindness, or rather the color blindnesses. There are quite a number of distinct types and subtypes of color deficiency and *in toto* they affect a surprising percentage of the male population. Color-blind women are not numerous, owing to the mode of inheritance of most of the defects; but about one man in every twelve is color blind in one way or another.

There is no reason to think that color blindness was any less common a hundred years ago, or a thousand, or ten thousand years ago than it is now. One might suppose that its existence would have become known to science as soon as there was science, and indeed one might expect to find it alluded to in the Bible. So, it is quite surprising that the first published description of a case of color blindness to receive any attention was written in 1777.

This was the case of "Harris the shoemaker," described by a country clergyman named Joseph Huddart in a letter to Joseph Priestley, who among other things was an authority on vision. Priestley published the letter in the *Philosophical Transactions of the Royal Society*. The shoemaker and several of his brothers suffered from the defect we nowadays call protanopia or red-blindness. One man in a hundred is a protanope, to whom red lights are dim (and actually have the hue of green), vivid red objects look like average green objects, and dark red objects appear like very dark

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green, dark brown, or even black ones. Harris, for example, could not tell blood from muddy water.

The publication of the Huddart-Priestley letter shortly called forth in England, then in France, and finally in Germany, a number of similar descriptions of color-defective individuals. From 1777 on, the literature of the subject grew rapidly. But when, during my preparations for research on color blindness, I worked back to the early history, I could not believe that such a conspicuous family of diseases had been totally unknown prior to the American Revolution.

It seemed to me that the ancients must have known about color blindness. Every civilization has made abundant use of color. The buildings and sculptures of the Greeks were richly painted, and not plain white at all like the plaster reproductions we see in our museums. Egyptian art was highly colored, with a climax in this respect during the reign of Akhnaton or Amenhotep IV, who set art temporarily free from religion. The dyes in use in the Middle Ages can actually be traced back to European prehistory. There were madder, woad, saffron, indigo, and kermes with which to make textiles just as gay as they can be made today with aniline dyes. What, then, about the one apprentice artisan in every twelve who mixed up his coloring materials, used red where he should have used green or used black where he should have used red? Why did he not make himself conspicuous? Probably he did; but probably his blunders were set down to stupidity and not realised to be due to a physiological defect. At any rate I could not find in the archeological literature, or in the history of dyes and dyeing, or in the rules and regulations of the medieval guilds any indication of an understanding that there were people who could never learn to use colorants correctly because they simply did not see them correctly.

Although I was forced to believe that color blindness had become known only in modern times, I did think that it should have been known before 1777 and I was still determined to try to be a historical detective. It had availed me nothing to jump all the way back to the ancients. It seemed that I would waste less effort if I started with the near-present and crept backward into time step by step.

The obvious place to start was in the bibliography of the *Handbuch der physiologischen Optik* of Hermann Ludwig Ferdinand von Helmholtz. When the last part of this great work was published

in 1860, Helmholtz had become the world's leading investigator of the physiology of color vision and color blindness. Surely he would have tracked down most of the printed words on those topics.

There were indeed a number of eighteenth-century authors cited by Helmholtz under the heading of *Farbenblindheit.* Any one of these writings might contain an allusion to some publication still earlier, which Helmholtz had missed in his own reading. Starting naturally with those which were in the library of my own university and easiest to locate, I took them out one by one and read them carefully. None gave the slightest clue to the existence of any description of a genuine case of hereditary, congenital color blindness earlier than that of Harris the shoemaker.

Some of Helmholtz's sources were harder to find. This was partly because he was a bit careless in some aspects of his work. There were all kinds of confusions in his references—wrong dates, wrong volume numbers, more than one "author" who turned out to be an abstractor or even the subject of someone else's writing. There were of course instances where the reference was accurate enough but outside the scope of local collections, which meant asking the interlibrary service department to borrow the publication for me elsewhere.

Eventually I located and studied every possibility in the 1860 Helmholtz bibliography excepting one. The reference to this I had kept aside for some time because it seemed incomplete and therefore likely to give me particular trouble. It lacked a date and seemed to pertain to a lay periodical which I could not locate in the *Union List of Serials.* Still, it looked perfectly harmless.

Harmless? I didn't know it was loaded—with more than two years of alternating frustrations and excitements, with one wild-goose chase after another leading me to neglect my more immediate professional tasks (and even my family).

The citation, to the plangency of which I was so innocently deaf at first, read:

> Giros v. Gentilly. Theorie der Farben (unter dem Pseudonym G. Palmer engl. und französ. erschienen). Lichtenberg Magazin. I. 2. 57.

It was only the sketchiness of this reference that made it seem exciting at all. The very fact that no date was given left it possible that Herr v. Gentilly had written prior to 1777. If so, he might have made some mention, so obliquely that Helmholtz had not caught

it, of some person he had known or heard of who saw colored things in some peculiar way.

Most of the excitement subsided when I took the next step. This was to check the second edition of Helmholtz, to see whether the reference was in any more complete form there. The second edition appeared in 1896 and was prepared by Arthur König, who had been Helmholtz's most brilliant student in the field of visual science. König tremendously enlarged the bibliography of the *Handbuch* to 7,833 titles, of which no fewer than 1,205 relate to the color blindnesses. Even so, he eliminated certain of the earliest references, which was why I had been using the first edition since my interest was not in the latest knowledge but in the earliest.

The Gentilly reference was not among those which König had weeded out, although as it turned out later I would have been a bit better off if it had been. König gave it thus:

1785

4237. GIROS V. GENTILLY. Theorie der Farben. Lichtenbergs Magazin. I. (2) S.57. (Unter dem Pseudonym G. PALMER englisch und französisch erschienen).

This did not help much. "Lichtenberg's Magazine" was either too much of an abbreviation or else not an abbreviation at all, for surely no scientific journal would have had just such a wording on its title-page. I still knew, or thought I knew, only that Gentilly had published an article in German in some German periodical, and that he had also published the same "theory of color" in English and in French, although for some reason under the pseudonym of "G. Palmer."

I now had a date, 1785, for the German publication; but this was not old enough to be anything but a disappointment. Still, all hope was not gone that Gentilly would be found to allude to something recognizable to a modern investigator (if not to Helmholtz) as color blindness existing before 1777. I had only to bustle about and locate some periodical which had the name "Lichtenberg" attached to it, and which was in business in 1785, and look at page 57 of the second issue in the first volume. The journal could not be hard to identify, if to Arthur König in 1896 the expression *"Lichtenbergs Magazin"* seemed unambiguous and sufficiently complete for it. By analogy, every physiologist knows what *"Pflügers Archiv"* is, so no one has to bother to write out *Archiv für die gesammte Physiologie des Menschen und der Thiere.*

Lichtenberg's Magazine was, indeed, easy enough to find. In the card catalogue of our university library, under the name Lichtenberg, there were a great many references. Most of them related to Georg Christoph Lichtenberg, who was clearly *the* Lichtenberg of all time. Professor of Physics at Göttingen from 1775 until his death in 1799, he was far better known as a satirist and epigrammist, dramatic critic, traveler, and all-around pundit.

The magazine of the Helmholtz reference was obviously the *Göttingisches Magazin der Wissenschaften und Litteratur,* of which Lichtenberg was chief editor. It was short-lived, its four volumes appearing over the years 1780-1785. The original Helmholtz reference was undated but called for the first volume. The date in the König version of the reference, 1785, was that of the last volume. I knew some little thing was wrong even as I walked through the stacks to find the set on its shelf. Standing there, I leafed fruitlessly through both the first and the fourth volumes, and ended by taking the whole set home.

There was nothing about color vision on page 57 of any issue of any volume. Pages 75, 157, 257—all the possibilities for which the "57" in Helmholtz might have been a misprint—were duds also. There was nothing for it but to look at every page of every volume. While this was an ordeal, it did give me considerable enjoyment. The "magazine of the sciences and literature" was a lot of fun. But it contained nothing about Giros von Gentilly or his Theory of Colors.

The reference in Helmholtz was somehow very wrong, and König had not made it right merely by attaching a date to it. "Lichtenberg's Magazine" must have been Helmholtz's shorthand for some other thing of which Lichtenberg had been editor. It had run to more than one volume, so that there were volume numbers, but the word *Magazin* was probably not actually in its title at all. The mass of Lichtenberg cards in the library catalogue offered several possibilities. I exhausted them, and vice versa.

The next thing that occurred to me was to look for writings with Georg Christoph Lichtenberg as the subject, not as the writer. Something of the sort, which had taken up more than one volume, might easily have been misremembered by Helmholtz as a serial and cited accordingly.

This tack led me to *Georg Christoph Lichtenbergs physikalische und mathematische Schriften,* edited in 1806 by L. C. Lichtenberg (a nephew) and F. Kries. Another set to carry home and go

through page by page. I shall always be glad that I did it, for on pages 272-273 of the fourth volume there was a surprising paragraph taken from one of Georg Christoph's diaries, undated but necessarily written down before 1799:

> Es ist doch ein äusserst sonderbare Sache, dass es so viele Personen gegeben hat, die die Farben nicht unterscheiden konnten. Ein merkwürdiges Beispiel davon steht in den Leipziger Sammlungen I. Bd. S. 637. Das sind wahre achromatische Augen. An der Linse und den Feuchtigkeiten kann es nicht gelegen haben, sondern in der Netzhaut und weiter einwärts. Warum man die Augen und das Gehirn dieser Menschen nicht untersucht? Solche Personen und ähnliche sind Geschenke, die die Natur dem Physiologen macht, und die er nicht annimmt. Ich glaub gewiss, die Verwandten eines solchen Menschen würden sich oft billiger finden lassen, als man glaubt. Der Staat müsste kleine Summen Geld dazu aussetzen.

This point of view was precocious by more than half a century. It was to take until the 1850's for anyone else to have the good sense to see that the study of the color blind would yield clues to the nature of the normal apparatus of color vision. Even then the tardy pioneer was not a physiologist, but the physicist James Clerk Maxwell. The problem of color occupied Maxwell for the first few years of his career, although that has been forgotten by the history-unconscious science of today.

All that Lichtenberg seems ever to have said anywhere about color vision or color blindness is that single astute comment in his diary. It seemed far-fetched, but still possible, that Lichtenberg's "Leipziger Sammlungen" was Helmholtz's "Lichtenberg's Magazine." The reference in the diary was patently an extreme over-abbreviation which Lichtenberg would not have used if he had thought he was writing for publication. For S. 637 to become corrupted to S. 57 was a mere nothing, if I know Helmholtz and his general bibliographical carelessness. So, I more than half hoped to find Giros von Gentilly's *Farben* on the pages of a *Sammlungen* of some field or other, published in Leipzig.

The obliging card catalogue, with its brazen tongue in its oaken cheek, first led me wrong with the *Leipziger Sammlungen von wirthschaftlichen Policey- Cammer- und Finanz-sachen*. The first volume of this was dated 1744, but the only mention of color vision it contained was in a metaphor about "a blind man trying to judge color." Wrong *Sammlungen*. The catalogue came up with three other wrong ones, one after another. From the *Union List* I learned of still others; and in addition some which, although primarily published in other cities, had had Leipzig editions.

For me to get a look at the first volume of all of these meant much labor for the interlibrary service department and in each case a wait of weeks. One by one they were received, examined, and ticked off until finally the *Sammlungen zur Physik und Naturgeschichte von einigen Liebhabern dieser Wissenschaften* arrived from one of the two American libraries that own it. Its date was 1779. Sure enough, on page 637 there commenced a discussion of color blindness—the one Lichtenberg had thought noteworthy. But this was only a German translation of the Huddart-Priestley letter in the 1777 *Philosophical Transactions*. All roads led me back to Harris the shoemaker. There was no evidence that Georg Christoph Lichtenberg had known of Giros von Gentilly or his *Theorie der Farben*.

To me this was baffling. Helmholtz could be wrong in the details of a reference, but how could he be so very wrong as to place von Gentilly's paper in a journal connected with Lichtenberg, who, although interested in color vision, apparently had never heard of Gentilly or his theory? A stout door seemed to have swung shut across my path to Giros von Gentilly. I gave up—for only the first time, however—an apparently hopeless task and resumed my reading of more accessible authors in the ancient history of the general problem of color.

Eventually I got around to the 1837 monograph of A. Seebeck, famous son of a famous father. He was one of the three or four most important investigators of color blindness in the pre-Helmholtz period. In his time, the relatively mild defects known as "anomalous trichromasies" had not yet been discovered. And, while total color blindness was assumed on theoretical grounds to exist, it is probable that no genuine case of it (or of the relatively rare "blue-blindness" either) had as yet been seen by any scientist. Color blindness, in the Seebeck era, therefore meant "daltonism," in which the victim sees only two hues in the whole spectrum instead of nearer two hundred. He consequently confuses red and green things since for him these have one and the same hue, and he confuses blue and purple objects since these also have one hue.

Seebeck's one great contribution in this field was making a distinction between two classes of daltonists on the basis of the particular confusions they made when, for example, they were asked to sort and match up the skeins of yarn in a jumbled pile. Seebeck's Class I contained the persons whom today we would call deuteranopes, while those in his Class II would nowadays be known

as protanopes. On the average, in every hundred men there is one who is deuteranopic and another who is protanopic.

Seebeck had only a dozen cases of his own; but, having established from them the existence of his two classes, he proceeded to try to assign to one class or the other every case he could find in the existing literature. For some of these the data given were scanty, or incorrect, or ambiguous. Other cases exemplified kinds of color deficiency which, in 1837, were not yet realized to exist. Among these earlier cases which Seebeck found he could not classify there was one which he listed simply as "the case mentioned in Lichtenberg's Magazine, I."

These few words of Seebeck's were the yank on the line that set the hook. I might never cross the trail of Giros von Gentilly again, but I knew if I ever did I should have to follow it as far as it might lead me. A "Lichtenberg's Magazine," containing an article on color vision and color blindness, did exist after all. When Helmholtz was beginning to work on the problem of color, he learned a great deal from Seebeck's monograph. It now appeared that it was from Seebeck that Helmholtz learned of von Gentilly's early description of a case of color blindness. The frustrating contraction of the real name of the journal it was in was not Helmholtz's doing at all, but Seebeck's. Under the smog of the Helmholtz-König reference there was positively some fire. What "Lichtenberg's Magazine" could be I had no idea, since it was not the *Göttingisches*. The mention in Seebeck named no author, let alone Giros von Gentilly. I was really no closer to meeting the latter and learning what he had had to tell about color blindness. But I felt somehow encouraged. The time I had spent on him so far did not seem so thoroughly wasted.

I went back to my program of reading and abstracting the early literature. In due course I came to study carefully the two monographs of Élie Wartmann. A Swiss, he was generally considered to be the world authority on color blindness until Helmholtz revived Thomas Young's three-component theory of color vision and interpreted the color blindnesses in terms of Young's ideas. Wartmann's masterwork, *Mémoire sur le daltonisme,* was published in 1843 and was translated into English in 1846. I was plodding through this *Mémoire* one evening, abstracting the text and copying out references as I went along, when I came to a paragraph which read thus:

"Girod de Chantilly parle entre autres (*) d'un pharmacien de Strasbourg qui ne distinguait que difficilement les couleurs. Il explique cette anomalie en admettant qu'il n'y a que trois couleurs, et que la rétine possède pour la perception de chacune d'elles une membrane particulière qui peut, dans certains cas, devenir inactive." The footnote mark led to a reference, in this form: "Dans un écrit publié en anglais sous le pseudonyme de G. Palmer, puis traduit en français avec le titre de Théorie des couleurs et de la vision. 8°."

Wartmann's "Girod de Chantilly" could not possibly be anyone other than Helmholtz's "Giros v. Gentilly." Moreover, here was very much more information about the man and his work. He had not written in German as Helmholtz had implied, but in English, and his work had subsequently appeared in a French version. It had not been published in a periodical at all, but was an octavo book or pamphlet, privately printed. It had dealt with color vision and not merely with *Farben* in the physical sense of coloring materials. Most important by far, de Chantilly's opus had offered a three-component explanation of color vision years before Thomas Young's and explained different kinds of color blindness by postulating the inaction of one or another of the three components.

Such a body of thought was years ahead of its time even in 1785, where König placed it. I had to find it and study it, not merely for its historical values but for the interest it would hold even for present-day researchers on color vision. There was plenty of collateral evidence that Helmholtz had read Wartmann's writings, but it was clear that at the time Helmholtz had gotten very little out of this statement of de Chantilly's surprising theory. Helmholtz had been so inattentive that he had dictated only a sentence or two to an amanuensis. The latter had caught "Girod de Chantilly" phonetically as "Giros v. Gentilly" and the scanty notes had later been organized into an entry for the 1860 bibliography. Subsequently, Helmholtz had overlooked the error of names through all the stages of the making of his book.

No wonder I had not been able to find any catalogued writings of "Giros von Gentilly" in standard sources. No one of that name had ever existed. It was bad enough for my quarry to have published under a pseudonym, but Helmholtz's carelessness had given him another one and had led me to expect to find something in German under that pseudo-pseudonym. All would soon be well,

however, for of course it was going to be a simple matter to locate Girod de Chantilly.

This meant going to the great French encyclopedias, which served as the model for the original *Britannica*. So exhaustive are they that if my man, as a scientist, had been of the least importance, he would be there. He wasn't. The only "Chantilly" was the little medieval walled town of that name, having some reputation for lace-making. There was no evidence that "de Chantilly," as a family name, had ever existed. Something was radically wrong, and I sat there awhile in the cavernous reference room to think what it could be.

The man's name, certainly, was Girod de Chantilly rather than Giros von Gentilly, *if* it was either of these. That seemed sure, inasmuch as Wartmann had certainly read the "Theory of Colors and Vision," whereas there was no evidence that Helmholtz or König had done so. Only Wartmann, then, had actually seen the name printed on the pamphlet. But it suddenly struck me that the "de Chantilly" was not necessarily a part of the name at all. The whole had occurred, in Wartmann's monograph, *running in French text* and with no difference in typography from the rest of the sentence. Very likely the man's entire surname was just "Girod." He had hailed from the town of Chantilly, however, and Wartmann had written "Girod de Chantilly parle . . ." just as we might write "Carlson of Chicago says . . ."

Back to the French encyclopedias to look for Girods. In *"La Grande,"* not a single Girod. In the middle-sized encyclopedias, no Girods either. But finally in a tiny encyclopedia, a brief entry for:

Girod, Amury (?-1837)

I sat down to read about Amury Girod. I shortly sensed something queer about the whole double page before me. I flipped the pages of the volume, looked again at the spine and the title page. This was an encyclopedia in French, yes; the *Encyclopédie Grolier*. But it had been written for French Canadians and published in Montreal.

I seemed to be far afield. Even so, the little that was there about Amury Girod sent me in haste to the Canadian history section of the library stacks. The fames and shames of French Canada are hunched together there. Amury Girod was one of the shames. No one book told me any more about him than it thought it had to. It took a while to piece together the story.

In 1837, the year of his death, Amury Girod was of an unknown age. If he had published a bright pamphlet on color in 1785 as a youth little past his schooling, he was perhaps 70 when his head was lopped off.

When Girod crossed the Atlantic and appeared in Canada, he was vague about the details of his past, but not at all reticent about his major accomplishments. He claimed to be an all-round scientist, and also something of a soldier of fortune. He had, he said, fought under Napoleon. It is a matter of record that Girod knew both French and English well enough to write magazine articles in both languages. Some of his analyses of Canadian affairs were accepted by periodicals in the old country.

Near Quebec, Amury Girod founded the first school of scientific agriculture in Canada. It did not last long, however, and Girod moved to a distant village. There he wooed and won the mayor's daughter, which set him up as a loafer for the rest of his life. Action, however, caught up with him. In the 1830's, Girod's village lay in the center of an intensely French area of Lower Canada in which resentment of British rule was rising toward a boiling point. A patriotic organization (inevitably calling themselves the Sons of Liberty) formed an "army." In this army, Amury Girod's loud mouth made him a general.

In the whole of the Rebellion of Lower Canada the rebel army fought only a couple of engagements, and these were of about the magnitude of our own Boston Massacre and Battle of Lexington. Only a few shots were fired, and there were only a baker's dozen of casualties. Then the inept rebels managed to get themselves surrounded. They were encamped, on a particular evening in 1837, with the British redcoats tented in an intact circle around them. For the Sons of Liberty, come the morn, there was no way out of a pitched battle which they could not win, unless they surrendered and took their chances on being hanged as traitors to the Crown.

Late that evening General Amury Girod made his way around the camp ordering his final dispositions. After midnight, he was back in his own tent; but he could not sleep—his feet were too cold. Finally he quietly arose, dressed, and stepped out in the dark to a row of picketed horses. Girod happened to steal a very good animal. He was through the patriot sentry-line, and the whole British encirclement as well, before anyone knew what was going on.

The awakened Sons of Liberty ran to tell the general of the commotion and soon figured things out. Girod had only a few

minutes lead over a posse that pounded down the highway behind him, but not for many miles did they catch him. It was close to dawn on a fine spring morning. Not a word was spoken that any historian has chosen to quote. A single saber stroke, and Girod's head and carcass were shoved into the ditch by a dozen booted feet.

The end of the Rebellion of 1837 seemed another end to my trail. I did not know positively that Amury Girod was the man in the Helmholtz bibliography, but I seemed to have come to him by logical steps and there was certainly no other candidate in sight. I could not see how I was ever to learn any more. The gods knew that I had tried to do my duty as a scholar. But I had to give up—again—without even a reference in good enough shape to be worthy of the name.

All this time I had been getting help and suggestions from three particular reference librarians who rotated on duty in the main reading room. They are among nature's noblewomen, like all reference librarians (who never ask a researcher whether what he wants is "important"). But just because they were three separate individuals, it was easy for each one to suppose that one of the others had probably already suggested this move or that one to me. The result was that one important source of bibliographical information fell in the middle, so that I remained unaware of it for nearly two years.

One of them happened to mention it at last, when I was standing by her desk one evening and we were going back over the whole history of my fizzled mission. What she said, musingly, was: "And of course you checked the Bibliothèque Nationale?" No, I hadn't; I didn't even know what it was. She then led me through rooms and corridors to a row of 177 bright red leather volumes—the *Catalogue Général des Livres Imprimés de la Bibliothèque Nationale* of France, in Paris. This is a counterpart of the catalogue of our own Library of Congress. It is not even complete, but fortunately for me it is finished through the P's. I looked under Girod, von Gentilly, Giros, de Chantilly, Gentilly, Girod de Chantilly, Giros von Gentilly, Chantilly. Nothing, anywhere. Now of course American libraries never list anything under a pseudonym—somehow they are always able to learn what a man's real name was, and it is under that name that his writings are indexed. I supposed that the national library of France would do likewise. Fortunately, however, I took the trouble to look under "Palmer." Glory be, there it was:

Palmer, G.
———Théorie des couleurs et de la vision, par M. G. Palmer. Traduit de l'anglais par D.-B. Quatremère d'Isjonval. Paris: Prault, 1777. 8°, iii + 56 pp.

So this was the publication Helmholtz had listed as *Theorie der Farben* and had placed in a German periodical! When the interlibrary service department opened for business the next morning, I was waiting at the door. From past experience, I knew that my helpmeet there could borrow blood from a turnip (for not more than two weeks). But when it came to cabling the Bibliothèque Nationale to pop a pamphlet onto the next Air France flight to the United States, she balked. There was, I learned, a ritual to be followed. We had to be able to assure Paris that there was no copy of the wanted item in this country. How were we to determine that? Ah, that was a routine chore for the Library of Congress.

When a library wants to find the unfindable for a researcher patron, it puts the reference in a letter to the Library of Congress. There, someone prepares 60 copies of a "have you got this?" letter. These go out to 60 American libraries notable for the size of their collections. We would have to wait for the Library of Congress to receive 60 replies from 60 overworked librarians. Then we would hear from the Library of Congress.

We waited; the seasons passed; we heard. Two American libraries, both in Philadelphia, owned copies. One had the French version, the other a copy of the original publication in English. At this point, despite Wartmann and his "Girod," it did not seem that "G. Palmer" could have been a Frenchman. Why would he have had to get someone else (d'Isjonval) to translate his English into French? But if his real name was Giros von Gentilly, and he was German, why did he write in English—and how good was his English? A German's English would still be better than some Frenchman's idea of what a German was trying to say in English. Without hesitation I dismissed the French publication, and the Philadelphia Free Library was asked to lend the English pamphlet. It came shortly, and I sat down with it in considerable excitement.

The 46 pages of Palmer's pamphlet were one continuous surprise. Quite apart from its scientific content, there was the matter of its organization. It is in the form of a dialogue between a "Johnson" and "Palmer." Just those surnames—no given names, nor even initials. The whole is practically a lecture, with Palmer doing most of the talking. Johnson's contribution consists of a few feeble defenses of the Newtonian system of color, which Palmer readily

THEORY

OF

COLOURS and VISION.

By G. PALMER.

LONDON,
Printed for S. LEACROFT, at the *Globe*, *Charing-Croſs*.

MDCC LXXVII.

FIG. 1.

refutes with the experiments he describes. In short, Palmer wins a long, one-sided argument, and Johnson has to admit that Palmer's intricate theory of the nature of light and the makeup of the mechanism of color vision must be correct in every detail.

For me, Palmer's theory itself was something tremendous, as I shall make clear. Equally intriguing was the form in which it was set forth. I had a hunch that I knew why my man had chosen to

print his theory under a false name. I went to the office of the Department of English and asked whether there happened to be on its staff any instructor who was supposed to know a great deal about the life and times of Samuel Johnson. Indeed there was; a Professor Bronson taught a course entitled "The Age of Johnson."

I obtained a word with Bronson and lent him for overnight my photostats of Palmer's pamphlet. I tried not to put any ideas in his head, but I need not have worried. When I went back to him next day, Bronson needed no prompting to agree with me. Yes, some of the sentences put in the mouth of "Johnson" were worded exactly as if Samuel Johnson had written them. There was no doubt that Palmer had used Samuel Johnson to make his own theory look good to his readers by having such a savant object initially to its various facets and then finally accept them. And if Samuel Johnson could have identified an author who made him lose an argument or pictured him as a fawning fool, Johnson would very likely have horsewhipped him.

In order thus to elevate the prestige of his theory by its own bootstraps, the dialogist had apparently gone so far as to hide behind a pen-name. If not, what name was false and what was true? "Palmer" was surely not French, for he could not gallicize his own English. For this reason his real name was probably not Girod de Chantilly. For the same reason and still another, he almost certainly was not Amury Girod. The general who lost his head twice in one night had seemed a possibility so long as the only date I had for Palmer's theory was König's "1785." But the real date was 1777 and to have been even twenty years old then, Amury Girod would have had to be eighty in 1837—a bit old for active rebellions and fast horses.

Then, Palmer's English was too good for him to seem a German. It was hard to believe that in Samuel Johnson's own period a German-born man with a name like "Giros von Gentilly" could have so effectively imitated Johnson's style. Only with the perspective of time and with much study of Johnson's writings long *ex post facto* would this have been possible for a foreigner. The Germanic "Giros von Gentilly," I was now sure, existed only as a mistake in a stenographer's notebook. At this point I had begun to think that while Palmer was perhaps not the theorist's real name either, he was at least English born and bred.

The "Theory of Colors and Vision" itself is absorbing, and I commend it to my fellow investigators of color vision and color

blindness. This is not the place for a full abstract of it and it is available, and in English, to interested scientists. Its historical position should interest any educated person, however. For an appreciation of that, it is necessary to speak first upon the color theory of Thomas Young.

Thomas Young (1773-1829), one of history's half-dozen universal geniuses, presented his theory in two lectures before the Royal Society in 1801 and 1802. He accepted Newton's idea that light is "vibratory" but he could not believe that at every retinal point there are a host of particles respectively resonant to each and every frequency. He proposed that there are only three kinds of microscopic receptors, each resonant to a big stretch of the spectrum, with little overlapping of the three regions. Agitation of the receptor types aroused respective elementary color sensations of red, green, and violet. All other colors and white were blended from these sensory components. The yellow and blue parts of the spectrum were exceptional in that each stimulated two kinds of receptors, so that yellow and blue sensations were synthetic even when aroused by physically pure ("monochromatic") light.

This hypothesis of Young's, strangely, was in a continuous state of rejection by everyone else until close to 1850, when Maxwell adopted it. It was never really "lost," however, for it was incorporated in Young's 1817 article on "Chromatics" in the fifth edition of the *Britannica.* That article stood unchanged even into the eighth edition, which came out during the very years (1853-1860) in which Helmholtz was "rediscovering" Young's hypothesis and doing experiments to validate it. Helmholtz, long ignorant of Maxwell, supposed that no one knew of Young's theory before he himself stumbled onto it in Lowthorp's abridgement of the *Philosophical Transactions.* Curiously, Helmholtz at first (1850-1852) rejected the theory scornfully. Then he suddenly embraced it enthusiastically. He never really understood it, perhaps because he was an uncertain translator of English. Helmholtz always thought that Young's receptors were each supposed to be somewhat responsive to the whole spectrum, which was decidedly not Young's notion of them. With this permanent distortion, the theory has been known to this day as "the Young-Helmholtz theory."

Helmholtz credited Young with being the first to say that color is sensory and subjective and not a physical property of lights and objects. He also thought that only Young had realized that color vision need be based upon no more than three fundamental hue

sensations. But Christian Ernst Wünsch (1792) probably deserves equal credit for this combination of ideas, if not also Tobias Mayer (1758). And Palmer had "Johnson" say that "most philosophers are agreed" that the color of a light is sensory and is not in the light itself.

While I was reading the Johnson-Palmer dialogue and Palmer's theory unfolded before me, I was soon comparing its tenets with those of Young's and wondering why, if Helmholtz knew the gist of Palmer's ideas, he had ever credited Young with having them first. Palmer's theory was just as complete as Young's and nowise inferior to it, considering that both were based upon naïve physics.

The true nature of the initial events in the illuminated retina is photochemical. Palmer thought it to be physical. So did Young. Palmer believed in tuned resonant vibrations of three kinds of receptoral elements, responding to three corresponding portions of the light in a white beam. So did Young. Palmer thought a white sensation to be the result of uniform agitation of all three kinds of receptors, with color resulting from an imbalance of stimulations. So did Young. For Palmer, colors were of the mind, and not physically present in objects and lights. They were so for Young also.

Young's theory had "red" light stimulating redness receptors directly and thereby arousing a red sensory element. Palmer's theory, here, was more indirect although it was just as reasonable physiologically as Young's. Palmer thought that his prism and filter experiments proved that a red surface absorbs red light and reflects the other two kinds. These latter stimulate two corresponding retinal receptor types; but the resulting sensation is determined by the undisturbed receptors, which in this case are the redness ones. So, the sensation is red. The underlying physics was no worse than Young's, for Young was led or misled by his opticist friend Wollaston to a misconception of the spectrum which Newton would have scorned more than a century earlier. Young understood absorption better than Palmer; but Palmer understood spectra better than Young.

There was not a word about color blindness in Palmer's pamphlet. But note its date: 1777, the same year in which Harris the shoemaker became known to science. If Palmer was a regular reader of the *Philosophical Transactions*, he must shortly have kicked himself for having launched his theory only a month or two, perhaps, too soon. He would surely have seen that to account for the black appearance of red objects to Harris, he would only need

to suggest that Harris had been born without any redness-arousing receptors in his retina.

This failure of the 1777 paper to deal with the then new subject of color blindness sent me back to Wartmann. He had said plainly that "Girod de Chantilly" had spoken of several cases of color blindness and in particular that of a Strasbourg pharmacist, that he had offered an explanation based upon selective inaction of retinal elements, and that he had put all this into the paper on the "Theory of Colours and Vision," which I knew he hadn't. Wartmann had known enough about Palmer's publication to be able to mention that its signatures were folded octavo. Either he had had it in his hands, or else he was quoting from an extensive abstract which I had not run onto anywhere. How, though, had color blindness gotten into any abstract when it was not in the original?

Looking again at the reference in Wartmann's footnote, it seemed to me that its wording meant that Wartmann did not even know what the English title had been. If Wartmann had read the paper at all, he had read it in French, and the French version must be somehow different from the English one. I dug out my copy of the listing in the catalogue of the Bibliothèque Nationale, and everything seemed to clear up. It gave the length of the 1777 *Théorie des couleurs* as 56 pages. My photostat of the English text contained only 46. I did not suppose French to be so clumsy that an exact translation of English would be 22 per cent longer. The French translator, d'Isjonval, must have worked in some additional material which Palmer had uncovered too late to put into the English publication. After the latter came out, no doubt, Palmer saw the Huddart-Priestley letter and was reminded of his defective friend in Strasbourg and of some other cases, and had sent some new pages to d'Isjonval. No doubt Palmer, feeling confident that Samuel Johnson would never see the French pamphlet, had also told d'Isjonval to reveal that his real name was Girod de Chantilly, so that Wartmann was taken in. It was just as possible, of course, that the two names were equally phony, with the "Girod de Chantilly" (or was it "Girod"?) intended to throw Johnson, or others in England, off the scent.

I went back to the interlibrary service department. Could I, please, borrow from Philadelphia the French pamphlet I now knew I should have asked for in the first place? Word came back from the library of the American Philosophical Society that their copy

had formerly been in the personal library of Benjamin Franklin and was too precious to be allowed out of their building. They would not lend it, but they were willing to microfilm it for me.

While I was waiting for the microfilm, my imagination filled those extra ten pages with all sorts of wonderful things. Palmer might even have blundered onto color blindness entirely independent of Huddart, although by coincidence in the very same year. Such things did happen. In any event the ten pages were sure to be a gold mine of lost cases, for if no one had noticed Palmer's paper in all these years, no one had known of all the cases of color blindness Palmer rounded up. I might even find that Palmer had found some seventeenth-century author, or someone earlier yet, describing genuine color blindness. I reminded myself that it was my attempt to break the 1777 barrier of color-blindness history that had caused me to look for "Giros von Gentilly" in the first place.

Then the microfilm arrived and the bubble was pricked. D'Isjonval had made a slavish translation and there was not one statement beyond what had been printed in English. There was no assertion that "G. Palmer" had been only the pen name of a Girod of or from Chantilly. Instead of an appendix at the end, describing *"entre autres"* the color-blind pharmacist in Strasbourg, there was at the beginning an impressive legal document occupying four pages and known as "the King's Privilege." This *imprimatur,* not obligatory in 1777 for a scientific publication, nevertheless carried with it some prestige, and Palmer may well have secured it for that reason. Following this were four more pages devoted to an editor's foreword, a letter of transmittal from Palmer to the editor (d'Isjonval), and a French translation of that. The remaining two pages of difference between the lengths of the English and French accounts of the theory were due only to the relative volubility of the French idiom.

Palmer had transmitted his text to d'Isjonval on 14 January 1777. D'Isjonval had actually already made the translation, in England, probably in the preceding year. When giving his final approval for the French printing, Palmer had not yet seen the Huddart-Priestley letter. In 1777, then, Palmer did not show any knowledge of the existence of color blindness.

There was nowhere to go but back to Wartmann. Where had he learned so much about what Palmer had said somewhere, sometime, about color blindness? In the 1777 papers, Palmer had had

Johnson address him as a "chemist," and had mentioned a future "Chemical Treatise upon the Art of Dying." He also promised another discussion with Johnson, in which he was going to explain fully the paradoxical production of black cloth with chromatic dyes. I could not find that either of these promised writings had ever been written. They were empty promises, fictitious parts of the build-up of Palmer as a great scientist in the Johnson dialogue. It must have been in something aside from all these things that Palmer ever wrote anything about color blindness. Unless Wartmann had known Palmer personally, he must have read something Palmer really did publish after 1777, or he must have read a second-hand account of such a publication.

There was only one thing, one appalling thing, to do: read in full every piece of literature cited in Wartmann's own monograph, on the chance that some one of them referred to some post-1777 paper of Palmer's—which would surely turn out to be in French, and under the name of "Girod" or "Girod de Chantilly." I would then find that Wartmann had mixed up this reference with one to the 1777 French paper, and in this way had mistakenly placed the Strasbourg pharmacist and the other color-blind people in the 1777 *Théorie des Couleurs et de la Vision.*

It took considerable time to read everything Wartmann had read, and it was all wasted so far as getting farther with the Palmer story was concerned. None of Wartmann's sources had ever heard of Palmer. Wartmann had not read about the Strasbourg pharmacist in anything he listed in his own bibliography. For all I could tell, Wartmann *had* known Palmer personally and had discussed color blindness with him face to face.

It was some time after this that I first had a horrid thought, which kept coming back. It was a disturbing recollection that somewhere or other I had already seen a reference to a publication of Palmer's much later than 1777. For weeks and months, I tried unsuccessfully to recall where this had been. Then it came to me, and I scuttled back to the catalogue of the National Library of France. When I had first looked there and found the 1777 Palmer reference, there had been another one beneath it to which I had paid no attention, because of its uninteresting title and particularly because of its late date. I had not even copied it out; so, it had been all too easy for me to forget that I had ever seen it. That overlooked listing read:

Palmer, G.
———Théorie de la lumière, applicable aux arts, et principalement à la peinture, par M. Palmer.———Paris, Hardouin et Gattey, 1786. In-8°, 64p.

When I came to the date I recalled that König had attached a nearly identical date to the "Giros v. Gentilly" reference and had listed the latter under the banner of color *blindness*. If it had been easy for König to confuse this paper with the 1777 one, perhaps it had been equally easy for Wartmann to mix them up too. I was sure that I would find the Strasbourg pharmacist and all the rest in this *Théorie de la lumière*.

Through the Library of Congress and its 60-fold correspondence with other libraries, we established that there was no copy of this 1786 pamphlet in the United States. Even so, interlibrary service did not write directly to the Bibliothèque Nationale. Lately, a new protocol had become *de rigeur*. I should write to "University Microfilms" in Ann Arbor. The B. N. would deal with that lofty institution, but not with anything lesser.

Finally, a microfilm of the 1786 publication arrived. It was quite a piece of business. M. Palmer may not have been Amury Girod, but he was a scoundrel nevertheless. In 1786 he was in partnership with a M. Quinquet, "maitre en pharmacie," who was a thorough-going crook. Quinquet was important enough to be pilloried in the encyclopedias, where I learned that he had been a technical assistant to Argand, inventor of the famous burner which did for oil illumination something like what the Welsbach mantle was later to do for gas. Argand at first was unable to interest continental capital in his invention. He went to England. He stayed there too long, for in his absence Quinquet marketed the Argand lamp in France as his own invention.

Palmer's contribution to the skullduggery was interesting from both scientific and economic standpoints. He had concocted a stained glass filter for use as a globe over the Argand burner. The effect of this was to whiten the yellowish light from the burner itself, so that the lamp thus specially equipped emitted an artificial daylight.

Palmer probably wrote his 1786 pamphlet principally to push the sale of this special lamp. A copy was given away with each lamp sold, and the pamphlet contained a lengthy advertisement for the lamp which would catch the eye of anyone who bought the pamphlet for its own sake. The lamp cost 120 livres in the silver-plated copper version, painted here and gilded there, and guaranteed

non-smoking and non-leaking. It could be bought at Palmer's residence at 18, *Rue Meslée,* during November, December, and January; or, at any time, at Quinquet's shop in the *Rue du Marché aux Poirées à la Halle.* The sales appeal was that any artisan who worked with colored materials, and had to stop at sundown because the colors thereafter "looked wrong" by artificial light, could beat out his competition if he bought a Palmer-Quinquet lamp. He could then work as far into the night as he chose, produce more, and make his fortune.

The 1786 paper did contain some information of possible usefulness to painters. It contained a résumé of the color-vision theory of 1777. And it did discuss color blindness. On page 52 Palmer said:

> Il est assez évident que la rétine doit être composée de trois sortes de fibres, ou membranes analogues chacune à un des trois rayons primitifs, et susceptibles d'être mues par lui seul . . . Il existe des personnes qui, quoique voyant très-clair, ne distinguent pas de couleurs, et n'ont sensation que de plus ou moins de lumière, c'est-à-dire, de clair et d'ombre, parce que chaque classe des fibres peut être mue distinctement par chacun des trois rayons: de ce nombre étoit le poëte Colardeau. Il est impossible de remédier à ce vice de conformation, et généralement à tout autre quand il est grave: mais quand il n'affecte légerement qu'une ou deux classes de fibres, on peut y remédier aisément ainsi. . . .

This is a remarkable passage. In it, Palmer describes what we nowadays call atypical achromatopsia—total color blindness with normal visual acuity. He explains it by a failure of differentiation of the receptor types, such that all three respond alike along the spectrum, making the sensory color-components always equal so that only a white sensation can result. Exactly the same explanation is believed in by many investigators today. The fact that Palmer, like most of the pioneers, mistook severe partial color blindness (such as that of Colardeau) for total color blindness is beside the point. In explaining partial defects by the impairment of one, or at most two, of the three receptoral systems, Palmer was being as modern as possible. Deuteranopia is still best explained as due to identity of the spectral responsivities of the redness and greenness receptors. We credit this explanation to Fick and Leber, who independently offered it in the 1870's.

Not only was Palmer's three-sensory-component theory of normal color vision just as good as Thomas Young's, Palmer was prepared to explain more types of color blindness than Young ever knew existed. Young's only comment on color blindness was made in 1807 in reference to a single and famous case, that of John Dalton (whose name was employed for sex-linked dichromasy—dal-

tonism—until Sir David Brewster introduced the term "color blindness" as a substitute). In 1798 the father of the atomic theory of matter published a description of his own color defect, which was probably deuteranopia. Although the infra-red limit of Dalton's visible spectrum was at the normal wavelength, he thought himself blind to the red light of the Brewsterian triple spectrum and blamed this upon a fancied coloration of the fluids of his eyeballs. Dalton lived an unconscionable time longer, with the scientific world waiting to learn whether his vitreous humor was blue or not. After his death in 1844, Dalton's vitreous was found to be colorless. (And all this was really not so very long ago, considering that the father of the Mayo brothers learned his chemistry from John Dalton.)

Young, commenting on Dalton's explanation of his own supposed red-blindness, simply said that: ". . . it is much more simple to suppose the absence or paralysis of those fibers of the retina, which are calculated to perceive red. . . ." This explanation is still the best one we have for protanopia, which was Harris' disease although it was not Dalton's. But Palmer anticipated Young, with just such explanations, by twenty-six years.

Palmer's 1786 paper did not, however, mention the annoying pharmacist in Strasbourg. Only one case was singled out for mention there by name, that of "le Poëte Colardeau." The poet Colardeau? I had already met him—he had played a part in one of the comedies of error in the 1860 Helmholtz bibliography. I was to meet him again, and in most unexpectedly close connection with Palmer.

When I had digested the 1786 pamphlet, I took stock. I now probably knew all of Palmer's theory of color vision and explanations of color blindness that Palmer himself had ever actually published. He was, of course, still promising writings he never did write. In 1786 it was to be an atlas of color samples for the purposes of color specification in science and industry, based upon three primary pigments and offering 2,160 standards—something rather more ambitious than even the Munsell system we use today.

Where, though, was there anything in print about the Strasbourg pharmacist of whom Wartmann had somehow learned? Worse yet, I still did not know who Palmer was in the first place. He wrote in English. He apparently did not even know French. D'Isjonval had translated one of his things into French, and it was probably Quinquet who did the other. Both Helmholtz and Wartmann had said that "Palmer" was a pseudonym. Perhaps it was.

But there was only Helmholtz's say-so for thinking that "Giros von Gentilly" was a real name, and only Wartmann's word for it that "Girod" or "Girod de Chantilly" was genuine. Both Helmholtz and Wartmann had proved themselves capable of making mistakes, and neither of their names for him checked with the fact that English appeared to be Palmer's native (and only) tongue.

"Palmer" is as English as can be. It could not have been a pseudonym adopted to conceal nationality. If it was false and was used in 1777 as a shield from the wrath of Samuel Johnson, why was it still used in 1786 and on a French publication? Samuel Johnson had died in 1784. "Palmer" surely knew this, for in 1786 he was spending nine months of the year outside of Paris and probably in his native England.

I was about decided that the theorist's real name must have been G. Palmer after all, and that he was certainly English. An English what? Chemist? He talked a good game as chemist; but the only person who ever addressed him as a chemist was the fictitious "Johnson." A pharmacist could easily pose as a chemist in print. At some time Palmer had known one pharmacist well enough to know that he was color blind. He had gone into business with another pharmacist. With pharmacists thus peering out of the woodwork everywhere, I was inclined to think that Palmer was only an itinerant pill-roller himself, pretending in print to be a great dye chemist.

On the other hand Palmer had known, well enough to get some work out of him, Denis-Bernard Quatremère d'Isjonval. This man *was* a genuine dye chemist, who published in 1777 a book on the extremely complex subject of indigo. He also wrote books on cotton, spiders, thirst, and fever, and he had a pet theory that all civilizations and cultures had been shaped by the geographical distribution of fresh water. Quite a person for a mere wandering druggist to know intimately. Perhaps Palmer really was a trained chemist but if he was, he never wrote the promised books of which he should have been capable.

The result of my stocktaking was that I thought "G. Palmer" to be the real name of an Englishman, perhaps a chemist but more likely a pharmacist, something of a fraud and the partner of a real crook, who nonetheless had brilliantly conceived a theory of color vision that anticipated Thomas Young's. Save for the chance circumstance of Helmholtz's random readings, we should today be speaking not of the Young-Helmholtz Theory but of the Palmer-

Helmholtz Theory. Palmer had explained the major types of color blindness in a logical and modern way in 1786, while Young was never to account for more than the misinterpreted case of John Dalton, and then not until 1807. Palmer was supposed to have gone by other names, but I could not find that he had ever published under any of them, I could not be sure who had tacked any of them onto him, and I could not tell which if any was his real one.

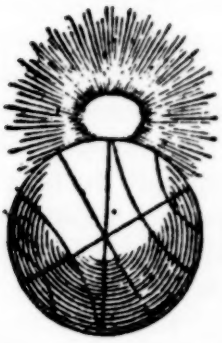
And now I was through. The story could never be finished; I tried to put it out of my mind. There were many other things for me to be doing if I were to round out my grasp of the early history of knowledge of color blindness. I was trying to do detective work on other cases also. There was the elusive one of the physiologist Unzer, mentioned by the phrenologists Gall and Spurzheim. There was the attempt to determine whether Dawbigney Turberville's "maid from Banbury" was an hysteric or the real thing. There were one or two others also living long before 1777, and any one of them might have been genuine and might have been described scientifically before Huddart wrote about the Harris boys to Priestley. I did find that in 1688 Robert Boyle described at least one real color-blind person. This was as far back as I ever did push.

One evening I was poring over one of my favorite sources of tips and clues to the history of all sorts of scientific things—Thomas Young's "Catalogue." This is a 431-page collection of references and abstracts of more than 20,000 works on science and technology which Young published in 1807 as a filler in the two fat volumes of his Royal Institution lectures. In one interesting abstract which I wanted to copy, the abbreviated name of the journal sent me to the list of complete titles, provided by Young, of the journals covered in the catalogue. As my eye drifted down column after column of these printed titles, the name "Lichtenberg" suddenly jumped out at me from the page.

Here was something listed as: "Gothaisches magazin für das neueste aus der physik und naturgeschichte, von L. C. Lichtenberg und J. H. Voigt. 8. Gotha, 1781." To Seebeck in 1837 this title might well have seemed to need severe abbreviation to *Lichtenbergs Magazin*. If this was so, then for more than two years I had been sidetracked by the wrong Lichtenberg's Magazine and the wrong Lichtenberg. What I had needed all along was certainly not the magazine edited in Göttingen by G. C. Lichtenberg, but one edited in Gotha by G. C.'s nephew.

Pres. by Wm. Maclure
1835

Magazin
für das Neuste
aus der
Physik
und
Naturgeschichte
herausgegeben
von dem geheimen Sekretär und Archivar
Lichtenberg
zu Gotha.



Erstes Bandes erstes Stück, mit Kupfern.

Gotha 1781.
bey Carl Wilhelm Ettinger.

FIG. 2.

The journal was not hard to find in the *Union List,* even though neither "Gothaisches" nor "Lichtenberg" proved to be part of its title. Through the interlibrary loan system, I soon had in my hands Volume I of the serial. König's date, 1785, was wrong even for this; the real date of the volume was 1781. But it was the right volume, and Helmholtz had had the right page number, 57 in the second issue. It was not a person nor a theory, but a publica-

tion cited in the 1860 Helmholtz bibliography that I had originally set out to find. At long last this was it.

The title-page (Fig. 2) explained much. The name of the serial was not much as given in Young's Catalogue. There were no initials for Lichtenberg, and Voigt's name did not even appear. No wonder Seebeck had alluded to this journal simply as Lichtenberg's Magazine. Young must have gotten his version of the title and editorship from some informed German source. I need not assume that Young had ever seen a whole volume of the journal, let alone that he had read from page 57 on in the first volume. Young *could* have seen the exposition, there, of Palmer's theory, but I would not insist that he did or that he consciously remembered it when he was evolving his own.

The long-sought article commencing on page 57 of the second *Stück* is signed simply "V." Everything else in the volume is signed either "V" or "L." So, the material on pages 57-61 was written by J. H. Voigt. It occurs in a regular department of the magazine which has a revealing subtitle of its own: *"Zur nähern Prüfung aufgestellte Muthmassungen, auch noch zweifelhafte Versuche."* The title of the article itself is: *"Des Herrn Giros von Gentilly Muthmassungen über die Gesichtsfehler bey Untersuchung der Farben."* At last I really knew where Helmholtz had learned about Palmer. Seebeck had given only the bare reference to *Lichtenbergs Magazin*. Helmholtz, reading Seebeck, happened to know what journal this meant. He found and read this article of Voigt's. From it he got the name "Giros von Gentilly." Meaning to refer to the latter's publication, he gave instead a reference to Voigt's article. Helmholtz gave no date for Giros von Gentilly's paper, for Voigt had not mentioned any. Helmholtz did not bother to amplify Seebeck's "Lichtenberg's Magazine," probably because he went from reading Seebeck to reading Voigt and scribbled what he got from Voigt into his abstract of Seebeck.

Voigt's article starts out with a mention of the color blindness of just two individuals, designated by name: Harris the shoemaker of course, and "Collardeau." Next comes the information that von Gentilly has encountered several similar persons, a particularly severe case being that of *"einem Apotheker in Strassburg."* Voigt then states that von Gentilly has laid a basis for his explanation of the phenomena of color blindness in his *Theorie der Farben und des Gesichts,* published in English and in French translation under the name of *Georg* Palmer.

The remainder of the article sets forth both the same receptor-inaction hypothesis Wartmann credited him with and the failure-of-differentiation theory Palmer was later to put into his 1786 paper, but here an analogy is drawn with the current theory of partial tone deafness. It is not too clear whether this comparison is being made by Voigt or was original with Palmer. Probably the former, for by coincidence (?) an article by Reimarus, drawing the same comparison, was on the point of appearing in the *other* Lichtenberg's Magazine (the *Göttingisches*). Palmer left the tone-deafness analogy out of his 1786 paper, which further indicates that it was not his own idea. Strange, however, is the omission in 1786 of the Strasbourg pharmacist.

The Voigt article closes with the same Palmerian promise of a "treatise on the chemistry of dyeing" which was made in the 1777 Palmer-Johnson dialogue.

Where, now, did Voigt get all his information about Palmer's thoughts on color blindness in 1781, in particular those which Palmer was to leave out of his 1786 paper? The 1777 paper, containing no mention of color blindness, was all Palmer had published at the time Voigt was writing. I can only suppose that Voigt learned several things directly from Palmer. Item, that the "G" on the 1777 title-page stood for "George" although "G. Palmer" was fictitious. Item, that Giros von Gentilly was the theorist's real name. Item, the case of the Strasbourg pharmacist. Item, the interpretations of color blindness. The only things Voigt could have gotten elsewhere were his knowledge of the color blindness of Harris and Colardeau and the tone-deafness analogy (which his editorial partner could have known of from his uncle).

Did Voigt get all this from Palmer in a face-to-face conversation, perhaps while on a trip to England? It is possible, but one little thing speaks against the idea. Voigt states that Palmer identifies the Strasbourg pharmacist as "M**." Such a thing is a little hard to do in a conversation, where the asterisks could not show. Writing about it later, Voigt would have had to invent his own printed symbol and would not have attributed this to Palmer.

I incline to think that Voigt got his special information from Palmer in writing. Wishing to report the 1777 "Theory of Colors" to the readers of his new magazine, Voigt probably wrote Palmer through his English publisher to ask whether there was any new material Palmer would like Voigt to work into his account. At the time, Palmer was excited about color blindness, infected Voigt,

and Voigt made this subject the centerpiece of his article. In the article, Voigt was probably quoting from a letter or from several letters, without bothering to mention the fact.

It is also not entirely certain where Wartmann got his information. From everything in the one paragraph in his 1843 monograph, one could think that he had gotten it all by reading Voigt and only Voigt except for three little things. How did Wartmann know that Palmer's 1777 publications were in octavo, when Voigt did not mention this? Why did Wartmann change Voigt's "Georg Palmer" back to the "G. Palmer" of Palmer's own title-pages? How could Wartmann, with Voigt's "Giros von Gentilly" before his very eyes, mangle this into "Girod de Chantilly?" Somewhere between Voigt and Wartmann there may have been a French abstractor who wrote an account based upon a reading of both Voigt and the original 1777 Palmer. And someone—though Helmholtz is now absolved—was careless about checking the notes of an earminded secretary.

It does seem to be settled where Helmholtz gathered what little he ever knew about Palmer. He may have noticed the paragraph in Wartmann about "Girod de Chantilly," but if so, it did not sink into his mind. Most assuredly Helmholtz did read Voigt's article, finding it by way of Seebeck. He gave a page number for the article, which Seebeck had not done. Helmholtz could never have read anything of Palmer's in the original, although he must then also have changed Voigt's "Georg" arbitrarily to "G." But even if Helmholtz was exposed to Palmer's hypotheses only through Voigt, the latter told so much about them that it is odd that Helmholtz never tipped his hat to them, either before or after he embraced the so-similar ideas of Thomas Young.

Who was responsible for the notion that Palmer's real name was Giros von Gentilly? I think it was Palmer himself, and that he invented the name for the special purpose of his correspondence with Voigt, for what reason I do not expect ever to know. Voigt believed what he was told (that "George Palmer" was a pseudonym but that "Giros von Gentilly" was a real name). I don't; but I haven't the least idea where Palmer got the "Giros." That name is not cognate to George, but rather to Gerald. Gentilly is no translation of Palmer, which means "a pilgrim." Despite the inserted "von," there is nothing German about "Gentilly." Its roots are in Italy where, as a family name, it has the form Gentili or Gentilli and means "of gentle birth."

We know that Palmer lived in France, but we do not know that he was ever in Italy. Gentilly is the name of a suburb, south of Paris, which in Palmer's time was still a tiny village. This may have inspired the pseudonym—but Palmer's part-time Paris address was in the northern part of the city. There is a more intriguing possibility as regards where Palmer got it—in France—when for some reason known only to himself he built the name into his false front.

The possible source of the "Gentilly" is embedded in the tale of the "Poëte Colardeau" whom Palmer mentioned as color blind in his 1786 article. In the color section of Helmholtz's 1860 bibliography there are two distinct-looking references which actually pertain to one and the same article. One is to "Rozier," as an author, and the other is to "Collardo," also as an author. "Collardo" was really a part of the subject matter of Rozier's article. Helmholtz did not create this mix-up, he copied it from Wartmann!

The Abbé François Rozier was the editor of an important French journal of physics, chemistry, natural history, and the arts. He was one of the first to be reminded, by reading the description of Harris the shoemaker, of a similar individual of whom he had heard. In 1779 Rozier retold the story of Harris in his own journal for his own readers and added an account of a man whose name he gave, on different pages, as "Colardeau" and as "Collardeau" and whom he called a painter. Rozier told several anecdotes to illustrate the kinds of amusing mistakes Colardeau had made in his efforts as an artist.

In 1786 Palmer called Colardeau a poet and Palmer was right. Charles-Pierre Colardeau was a minor literary figure whom his biographer, Bocate, called "the poet of the fields." Colardeau's works were the subject of a Ph.D. thesis written on my own campus a few years ago by Leonard Messier. In correspondence, I found that Messier did not know that Colardeau had ever painted anything, although it was well known that he did a lot of pencil sketching.

Aside from Palmer, only Rozier seems to have made an original statement to the effect that Colardeau was color blind. Voigt mentioned this, but spelled the name "Collardeau" as Rozier had done. This implies that Voigt's knowledge of Colardeau (and of Harris) came from Rozier's journal. But Palmer, who spelled the name only correctly, may never have seen Rozier's article at all. He knew Colardeau's name and his vocation better than Rozier did.

Is such slim evidence sufficient to indicate that Palmer knew Colardeau personally? Colardeau died in 1776. In his 1777 paper why didn't Palmer mention color blindness, Colardeau's color blindness, if he had known the poet in life? Maybe Palmer never knew him; but if he did not, there is considerable coincidence revolving around the name of "Gentilly."

Charles-Pierre Colardeau was always sickly. His sisters were even more so. From time to time the ladies got the urge to visit the French town of Segré, where there was a spring or fountain whose waters were reputed to be curative. Charles-Pierre did not believe in it. While his sisters were taking the waters, he avoided boredom by roaming the countryside with his sketch pad. It was on these walks that Colardeau made the acquaintance of the gardener of a large estate owned by one Jacques Leboeuf. He much enjoyed hobnobbing and philosophizing with this gardener.

Did "George Palmer" ever know Colardeau? Well enough to travel to Segré with him, or to visit him, at least once? When he thought he needed a new name, did he hit upon "Gentilly" because at the moment he was thinking about color blindness? Specifically, Colardeau's color blindness? Did he adopt the name while he was in the very act of writing a letter to Voigt? It would be nice to know the answers to these questions. For the name of Jacques Leboeuf's estate was:

GENTILLY.

Two Remarkable Roman Stone Weights in the Edward C. Streeter Collection at the Yale Medical Library

BRUNO KISCH*

WEIGHTS and scales, items of the highest importance in chemistry and pharmacy, have a history like certain other instruments such as knives and magnifying glasses, reaching farther back than the date of their introduction as tools of science and medicine. Among the treasures in the collection of weights and measures that the late Edward C. Streeter gave to the Medical Library of Yale University and the later accessions to the collection are two weights of special interest dating from Roman times which deserve a short description. One of them is a ten-pound weight in the usual shape of a segment of a sphere; the other is a fifty-pound weight of very unusual form.

Provenance and Description of the Weights

Both these weights were bought in Naples in a third-rate antique shop where they had been stored for many years in the back yard of the shop. They are presumably either from Naples, or more probably from Pompeii. The material is greenish black serpentine, which was often used for this purpose in Roman imperial times.**

The fifty-pound piece (Figs. 1-2) has the shape of an egg segment. Its greatest circumference presents an ellipse with a diameter of 245:210 mm. The bottom plane is an ellipse with a diameter of 175:150 mm., and the top plane, also an ellipse, has a diameter of 230:190 mm. The top surface shows that, like most heavy weights in Roman times, this fifty-pounder had had at one time a handle with which to lift it and put it on the balance. There are two cylindrical holes, one entirely empty, with a diameter of 50:45 mm. and a depth of 40 mm. At a distance of 40 mm. is the other hole, with a diameter of 45:40 mm. This latter hole, still filled with lead, shows that the missing handle was chopped off there.

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** I am obliged to Mr. Horace Winchell, professor of mineralogy and crystallography and curator of the Brush Mineral Collection of Yale University, for confirming the nature of the stone.

Whether the handle was of lead or of brass filled with lead can no longer be determined. The top plane also bears a mark—three letters formed by little dots to the height of 15 mm. The first is easily legible as a line, probably indicating an "I"; the second is clearly readable as a "P"; the third could be an "A" but it is not too definitely legible. Three dots are between the letters, as the following drawing indicates (Fig. 3). Above this mark, on the top, is a small square, barely visible, and in it a Greek cross. The height of the object is 138 mm., its exact weight 13,855 grams. That is equal to 42.3 Roman pounds, or 42 pounds, 3½ ounces.

Fig. 3. The inscription on the fifty-pound weight.

The ten-pound weight (Figs. 4-5) has the usual form, a segment of a sphere. Its largest diameter is 135 mm. The top plane has a diameter of 92 mm., and the bottom plane of 93 mm. The height of the entire object, which is in very good condition, is 96 mm. In the middle of the top plane there is a hole for a missing handle. On the bottom plane, in the center, is a Roman "X" marked with little dots, indicating the object to be a ten-pound weight. One branch of the "X" has four instead of three dots, which makes for a longer arm of the "X" mark and gives the impression of a Latin cross. The weight of this object is 3,218.5 grams (that is, 9 Roman pounds and approximately 10 ounces.

DISCUSSION

Stone weights of Roman times in the form of a segment of a sphere are not too rare, and the Streeter Collection contains several of these monuments of a bygone era in a very good state of preservation. This type of weight and dotted inscription is usual up to the fourth century, and many specimens can be found in the beautiful collection of the Museo Nazionale in Naples. The inventory of the Streeter Collection gives no definite indication as to whether or not the two weights really came from Pompeii, but it is very probable, and this would give us as a *terminus ad quem* the date of 79 A.D. when the three towns were buried by the eruption of Mount Vesuvius. However, placing the two pieces before 79 A.D. can be regarded only as an opinion.

In the case of the larger weight, taking into consideration the fact that a big handle is missing, the weight of 13,855 grams corre-

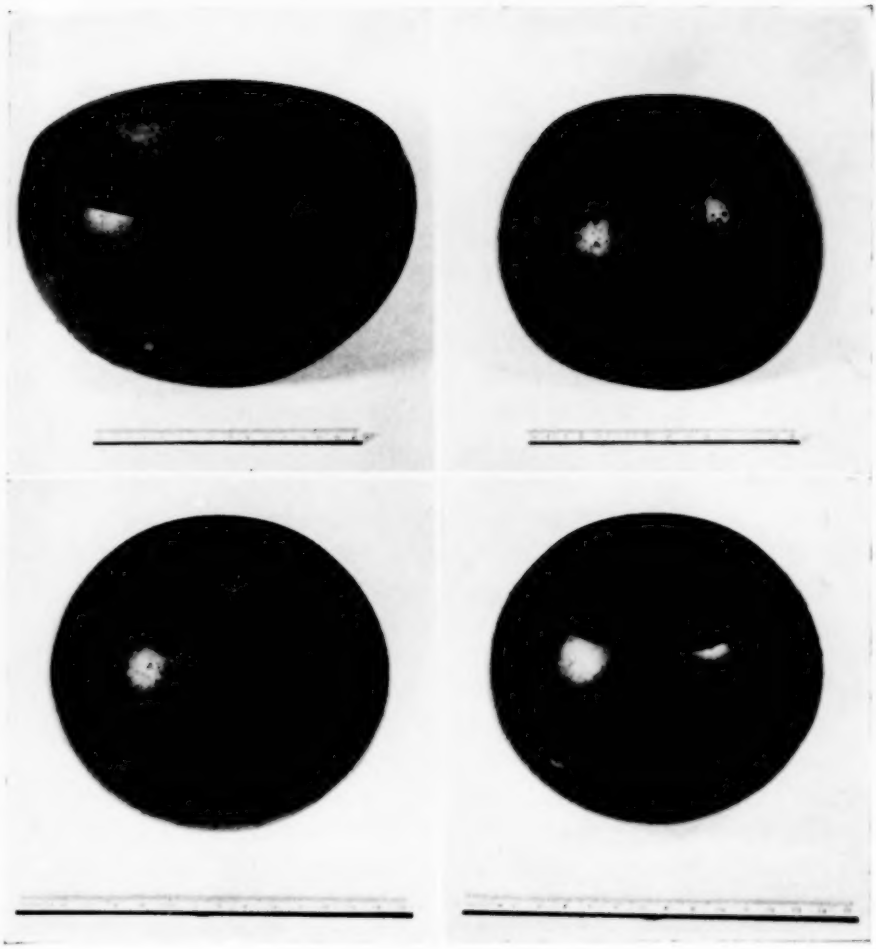


Fig. 1. The fifty-pound weight. On the top plane one sees the two holes where the handle was inserted. The hole on the left side is still filled with lead.

Fig. 2. The fifty-pound weight with the bottom side up. The photograph is taken from a 90° angle to that of Fig. 1.

Fig. 4. The ten-pound weight. In the middle of the top plane can be seen the hole where the handle was inserted.

Fig. 5. The ten-pound weight with the bottom plane up. One sees in the middle of this plane the Roman "X," indicating the weight of ten pounds. The elongation of one branch of the "X" and the fairly 90° angle of both branches give it the appearance of a Latin cross, which probably was not merely a coincidence.

sponds very well to the expected weight of 50 pounds, which would be the equivalent of 16,380 grams. The Roman pound in imperial times was 327.45 grams. Whether the inscription of three letters indicates the name of the owner or of the maker of this weight cannot be determined. The middle initial "P" could also stand for the word *pondo* (weight). The small cross on this plane indicates that the weight was in the possession at one time of a Christian, which would be improbable but not impossible before the year 79 A.D. Fifty-pound weights in stone are rather rare, even in the big collections. Karl Pink, who in 1938 published an excellent treatise on Roman and Byzantine weights with a catalogue of all such weights in Austrian collections,* has in his catalogue among the stone weights up to the year 300 A.D. only two fifty-pound weights registered, both of limestone, one in the form of a sphere and the other in the form of a sphere segment, but none of serpentine or in the form presented in our first picture. The two fifty-pounders he mentions weigh 14,900 and 14,930 grams respectively. He mentions two ten-pound weights of stone, both also of limestone and not of serpentine. The weight of the ten-pounder in the Streeter Collection fits very well to the exact weight of ten pounds, which should be 3,275 grams, and therefore lacks only 55 grams of the normal weight, which can easily be attributed to the missing handle of whose previous existence the hole in the center of the top is proof. These handles of heavy weights in Roman times were sometimes very elaborate, as can be seen in the collection of the Museo Nazionale in Naples. One such weight of bronze from Pompeii, which I was able to investigate through the kindness of the director of this Museum, Professor Maiuri, has a hole on the top and it weighs 29,600 grams. It may have been a Roman *centipondium* (a hundred-pound weight). Its handle represents two fingers connected by a ring in a nicely elaborated manner.

The elongation of the one branch of the Roman denomination "X" and the fact that the angle between both branches of the "X" seems to be larger than usual (see Fig. 5) would again hint at an early Christian engraver of the weight, just as does the little cross on top of the fifty-pound weight.

As Pink mentions in his little monograph, the idea of creating a catalogue (corpus) of all existing classic weights was already much discussed in the circles around the famous historian, Theo-

* Pink, Karl. *Römische und Byzantinische Gewichte in Oesterreichischen Sammlungen.* Baden bei Wien, R. M. Rohra, 1938. (Sonderschriften des Osterreichischen archäologischen Institutes in Wien, vol. XII.)

dor Mommsen, an idea very justly also supported by Pink, who at least produced such a catalogue of the weights in Austrian collections. If this present note should arouse interest in putting together a catalogue of all ancient weights and scales existing in the collections in the United States, it would be fortunate because such a catalogue would be a valuable contribution to the history of science. A catalogue of the weights in the Streeter Collection is being prepared at the present time and, it is hoped, will be published in the not too distant future.

ACKNOWLEDGMENT

I am grateful to Howard J. Reynolds, technical specialist in medical photography at the Yale School of Medicine, for making the excellent reproductions of these black weights which are so difficult to photograph.

Notes and Queries

Edited by Dorothy M. Schullian, Ph.D.*

A Portrait of Samuel Sharp

Samuel Sharp (1700?-1778), whose surname sometimes appears with a terminal "e," has been designated "the first great surgeon appointed to Guy's" and is remembered as the originator of a method of extraction of cataract.[1] His *Treatise on the Operations of Surgery,* London, 1739, went through at least ten editions and was translated into French and Portuguese. His *Critical Enquiry into the Present State of Surgery,* London, 1750, also appeared in several editions and was translated into French, Spanish, German, and Italian. A pupil of the great Cheselden, he studied as well in France and probably represents one of the chief sources of French influence on British surgery and anatomy. The course in anatomy which he once gave in Covent Garden to a society of naval surgeons was taken over by William Hunter early in his London career, and it is likely that Parisian example in the teaching of anatomy entered into the British tradition partly by this means. Sharp has been praised by Sir James Paget as a good representative surgeon, unscientific but "eminently safe." He deserves further attention from someone having access to source materials in London, and possibly in Paris, not because he was a great man, but because he was a good type of the pre-Hunterian eighteenth-century surgeon, a man who seems to have summed up in himself all that was best in the older tradition, and who yet had a rôle in the beginning of new things. He was also a man of parts, whose *Letters from Italy* were pronounced by Samuel Johnson to have "a great deal of matter in them."

Sir Zachary Cope, in his recent book on Cheselden, has lamented that although Sharp may be glimpsed as one of the figures in the frontispiece of his master's *Osteographia,* no true portrait exists. By one of fate's remote decrees, a likeness has nevertheless survived in rather improbable fashion. Jessé Foot, that serio-comic, atrabilious biographer and detractor of the mighty John Hunter, required a foil to set off his villain and thought he found the perfect candidate for the part in Samuel Sharp, "Legitimate Surgeon and Anatomist." It is for this reason that an elegant pencil drawing of the surgeon's right profile, executed in the manner of George Dance, finds a place in Foot's own copy of his *Life of John Hunter,* elaborately grangerized, and now preserved in the Wellcome Historical Medical Li-

* Cleveland, Ohio.

[1] Cope, Zachary. *William Cheselden: 1688-1752.* Edinburgh and London, 1953. Hubbell, A. A. Samuel Sharp, the first surgeon to make the corneal incision in cataract extraction with a single knife. A biographical and historical sketch. *Med. Libr. and hist. J.,* 1904, *2,* 243-268; also *Amer. J. Ophth.,* 1904, *21,* 289-315. Power, D'A., in *Dictionary of National Biography.*

brary. One pictures poor Foot, swelling with complacency and malice, inserting in these volumes the scores of drawings he had commissioned, many of them vicious caricatures of Hunter; adding the small number of pictures of the elect, the "legitimate" conservatives; and poring over them all as a miser might gloat over coin. Foot was a unique compound of envy, hatred, malice, and all uncharitableness; but thanks to these unamiable qualities much information has been preserved to us, together with a number of really admirable drawings. Among others is the portrait of Sharp. *Ex malo bonum.*

LLOYD G. STEVENSON

Tobias Smollett, M.D. and William Battie, M.D.

During research into the history of psychiatry in England in the eighteenth century we read Tobias Smollett's novel *Sir Launcelot Greaves,* first published in serial form in 1760-1761 and as a book in 1762. In it we came across a paragraph which seemed familiar and eventually discovered that it had been copied almost word for word from *A Treatise on Madness* by William Battie, published in 1758. This hitherto apparently unknown fact is obviously of literary interest in pointing out one of Smollett's sources. It is equally of medical interest, because Battie's position and influence in the history of psychiatry is not yet sufficiently appreciated.

Battie had been appointed physician to St. Luke's Hospital from its opening in 1751, the new foundation having been prompted by the inadequacy and abuses of Bethlem Hospital.[1] In 1753 he received the sanction of the governors to teach "gentlemen of the faculty" and thus became the first teacher of clinical psychiatry in Great Britain, if not the world, anticipating Alexander Morison at Edinburgh by almost seventy years and Bethlem Hospital by ninety. His *Treatise on Madness* was written at the request of his pupils, and was the first book on mental diseases in England.[2] He retired from St. Luke's in 1764, the year of his election to the presidency of the Royal College of Physicians of London, a rare honour for a "mad-doctor." Neuburger[3] remembers him as one of the earliest protagonists of Haller's doctrine of irritability, as shown in his Lumleian lectures published under the title *De principiis animalibus,* in London, 1757. It was largely due to Battie's evidence before a parliamentary committee in 1763 that the first act safeguarding the welfare of lunatics in private madhouses was passed in 1774.

The relevant passage in *Sir Launcelot Greaves* occurs in chapter 23, on pages 221-222 of the first edition (1762), and runs as follows:

1 Hunter, R. A. and Macalpine, I. "William Battie (1703-1776)," *Practitioner,* 1955, *174,* 208-215.

2 Laehr, H. *Die Literatur der Psychiatrie, Neurologie und Psychologie von 1459-1799.* Berlin, Reimer, 1900, vol. 2, p. 512.

3 Neuburger, M. *Die historische Entwicklung der experimentellen Gehirn- und Rückenmarksphysiologie vor Flourens.* Stuttgart, Enke, 1897, p. 159.

Artist unknown. From Jessé Foot's extra-illustrated copy of his *Life of John Hunter* (vol. I, p. 124) in the Wellcome Historical Medical Library.

(Reproduced by kind permission of Dr. E. Ashworth Underwood)

"Do you think my distemper is madness, doctor?"—"O Lord! Sir,—not absolute madness—no—not madness—you have heard, no doubt, of what is called a weakness of the nerves, Sir,—though that is a very inaccurate expression; for this phrase, denoting a morbid excess of sensation, seems to imply that sensation itself is owing to the loose cohesion of those material particles which constitute the nervous substance, inasmuch as the quantity of every effect must be proportionable to its cause; now you'll please to take notice, Sir, if the case were really what these words seem to import, all bodies, whose particles do not cohere with too great a degree of proximity, would be nervous; that is, endued with sensation—Sir, I shall order you some cooling things. . . ."

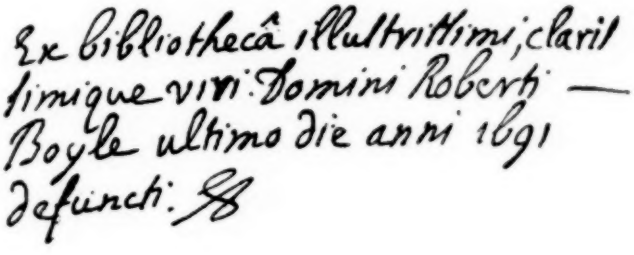
Dr. Battie wrote on pages 16-17 of his *Treatise on Madness* (1758):

. . . let us divert our attention to a very common phrase, *viz. weakness of nerves,* which . . . is . . . extremely delusive. . . . For since the word *weakness,* when joined with material substances, can convey no idea but a lax cohaesion of such particles as constitute those substances; therefore the phrase *weakness of nerves,* which denotes a morbid excess of Sensation, seems to imply that Sensation itself is owing to the loose cohaesion of those material particles which constitute the nervous substance, inasmuch as the quantity of every effect must be proportionable to its cause . . . if the case really were what these words seem to import, all bodies whose particles do not cohaere with too great a degree of proximity would be nervous, that is endued with Sensation.

IDA MACALPINE
RICHARD A. HUNTER

A Book from Robert Boyle's Library

The fate of Robert Boyle's library has been much under discussion since Dr. Douglas McKie (*Nature,* 23 April 1949) discovered that it had been partly catalogued and sold with the library of Silvanus Morgan on 5 April 1693. Several presentation copies of Boyle's own works are known, but



nothing else from his library, which was evidently large and varied, has ever been traced until a copy of *La Politique des Jesuites* was recently discovered. The book was published in London in 1688 and while unsigned was evidently written by Louis de Montpersan. It is bound with another anonymous work *La Decadence de l'Empire Papal* (Amsterdam, 1689), and the volume bears the significant inscription: "Ex bibliothecâ illustrissimi, clarissimique viri: Domini Roberti Boyle ultimo die anni 1691 defuncti." We are grateful

to Miss Leona Rostenberg of New York for bringing the volume to our attention. It is interesting that in Boyle's will he bequeathed funds to the French Protestant refugees. Boyle no doubt shared the views of the anti-Jesuit author of *La Politique des Jesuites* and for that reason had acquired the volume in question. The author of the inscription stating that the book came from Boyle's library has not been determined. The book bears the shelf mark "XLIX.K.22" which indicates that it was part of a large library.

Montpersan's book is listed in Bourgeois and André, no. 3099 and in Barbier, III, column 943-4. A photograph of the inscription which occurs on the fly-leaf is included herewith.

J. F. F.

A Letter by Hyrtl

The famous textbook by Joseph Hyrtl, *Lehrbuch der Anatomie des Menschen,* was first published in 1846 and passed through twenty-two editions and innumerable translations. The copy of the 1867 edition which is preserved in Texas in the Bexar County Medical Library at San Antonio is of more than usual interest because it contains a letter in the hand of Hyrtl himself. Written at Vienna on 17 October of an unstated year, it carried to the medical student Rudolf Menger at Leipzig the decision of the Vienna faculty on Menger's request to study at Vienna. Menger later practised at San Antonio. The letter is here translated as follows:

Most honored Sir,

I have presented your case to our Dean Dr. Prof. Braun and received word from him that there is no obstacle to your admission as "ordinary listener" of medicine in our university.

When you have heard all the prescribed scientific subjects, partly in Leipzig and partly here in Vienna, there is no objection to your graduation here after passing strict examinations. You have then only to present a *Revers* that you as a foreigner will not practise in Austria. (It is understood that you can receive a dispensation.) What your admission to the medical examination requires are certificates for collegiate subjects heard: Anatomy, Physiology, Mineralogy, Zoology, Botany, Chemistry, General Pathology, Pharmacology, Clinical Medicine, Ophthalmology, Obstetrics, Pathological Anatomy, Veterinary Arts, and Forensic Medicine. One studies here five years in order to hear all the subjects. You will meet here many of your countrymen. This is all that can be told to you by

Your devoted
J. Hyrtl

On the back of the letter is a quotation in another hand, possibly Menger's, from the *Leipziger Nachrichten* for August, 1872. It reads in translation:

Clinique Room Ornaments

Professor Hyrtl, the celebrated Viennese Anatomist, has established in Putholdsdorf a studio and a bedroom of whose originality the following notes give a preliminary taste.

Close to the cemetery stands an old ruin which Hyrtl converted into a studio and bedroom with no change in the outward appearance of the ruin. The studio represents a small anatomical museum which contains his rarest and most perfect specimens. Here are, as he expresses it, his "cabinet pieces." His bedroom, extremely plain, without decorations and very small, contains as ornaments one hundred skull skeletons, especially the heads of the most notorious robbers on earth, which Hyrtl has collected for more than forty years.

PAT IRELAND NIXON

A Note on the History of the Germ Theory of Disease

I should like to call the attention of readers of the JOURNAL to an interesting paper in the March 1955 issue of *Annals of Science* (*11*, 44-57) by Raymond Williamson, Reader in the Department of Pathology at Cambridge, entitled "The Germ Theory of Disease: Neglected Precursors of Louis Pasteur (Richard Bradley, Benjamin Marten, Jean-Baptiste Goiffon)." Mr. Williamson has analyzed the three volumes by the authors mentioned which all appeared within a year of one another: Richard Bradley, *A philosophical account of the works of nature,* 1721; Benjamin Marten, *A new theory of consumptions,* 1720; and Jean-Baptiste Goiffon, *Relations et dissertation sur la peste du Gevaudan,* 1722.

Collectively, Williamson writes, they are ". . . sufficient to show that [these three men] had fully accepted the hypothesis that certain diseases, especially epidemic ones, can only be explained on rational grounds by postulating the presence of a contagium vivum; that different diseases are caused by different organisms; that organisms may be conveyed directly from person to person, by air, by food, or by fomites; that once they have entered the body they can be carried by the blood stream to other parts; and that there may be an incubation period between the entry of the organisms and the occurrence of symptoms. These are some of the basic principles of pathology and these men were indeed pioneers of the Germ Theory of Disease."

J. F. F.

Dr. George Sarton Honored

On 29 December 1955 Dr. George Sarton became the first recipient of the Sarton Medal awarded by the History of Science Society for contributions to the history of science. The medal, named in his honor, was presented to Dr. Sarton by Dr. Dorothy Stimson, President of the Society, at its annual meeting in Washington. The awarding of the medal by the Society was made possible through a generous grant from Chas. Pfizer & Co., Inc. of Brooklyn.

Dr. Sarton, author of the five-volume *Introduction to the History of Science,* is professor emeritus of the history of science at Harvard where he was a member of the faculty for thirty-five years. He was the founder and for forty years the editor of *Isis.* He is president of the International Union

of the History of Science and has been honored in several countries abroad for his work in this field. He is an Associate Emeritus of the Carnegie Institution of Washington.

J. F. F.

Mozart's Illness and Death

Dr. Juhn of London, in the *Ciba Symposium* published by Ciba Limited of Basel, Switzerland, recently discussed the nature of the last illness of Wolfgang Amadeus Mozart who died at the early age of thirty-five after a lingering illness. In this bicentenary year (he was born on 27 January 1756), when there is great interest in every aspect of Mozart's life, this note on his last illness seems appropriate.

There have been various speculations concerning the nature of his illness. By some it had been thought to be tuberculosis, by others some obscure cutaneous disease, possibly erythema nodosum secondary to a hemolytic streptococcal infection. He was known to have had a severe attack of scarlet fever in 1762, and three years later he was in bed for three or four months at Lille with typhoid fever. In 1767 he came down with smallpox in Vienna, and in 1781 he was struck with pneumonia in Munich; in fact, he suffered from ill health throughout his life.

After sifting all available evidence, especially the signs and symptoms which were present during his last illness when he had attacks of dyspnea, convulsions, delirium, and severe edema of all four extremities, coupled with excruciating headaches, Juhn concludes: ". . . it is highly probable that Mozart was suffering from malignant hypertension with hypertrophy of the left ventricle, and that his death was caused by cardiac and renal failure." For some days prior to his death, which occurred early in the morning of 6 December 1791, he had complained of having the "taste of death" on his tongue, a common symptom in the late stage of uremia.

J. F. F.

Professor Adalberto Pazzini Elected to Chair of the History of Medicine in Rome

Early in December the competition for the Chair of the History of Medicine at the University of Rome was concluded. The jury, consisting of Professors Cazzaniga, Ciasca, Di Macco, Frugoni, and Puntoni, announced as the successful candidate Professor Adalberto Pazzini, who for approximately twenty years has been the professor in charge of this subject at the University. This is the first official chair of the history of medicine to be assigned since the unity of Italy.

Professor Pazzini held his inaugural lecture in January on which occasion he commented on the first aphorism of Hippocrates.

J. F. F.

Plaque in Memory of James Parkinson

As recorded in *Nature* for 24 September 1955, a memorial plaque to James Parkinson (1755-1824), one of the most eminent British neurologists of his time, was unveiled on 17 September in St. Leonard's Church at Shoreditch. The ceremony was presided over by Dr. J. Godwin Greenfield, retired neurologist of The National Hospital, Queen Square, who made the principal address. (See *James Parkinson, 1755-1824*, London, Macmillan, 1955, for the full proceedings.)

IN PIAM MEMORIAM

JAMES PARKINSON

OF HOXTON SURGEON AND APOTHECARY

DIPLOMATE OF THE COMPANY OF SURGEONS
MEMBER AND THE FIRST HONORARY MEDALLIST
OF THE ROYAL COLLEGE OF SURGEONS
IN HIS "ESSAY ON THE SHAKING PALSY"
HE FIRST DESCRIBED THE DISEASE
SINCE KNOWN BY HIS NAME
FOUNDER MEMBER OF THE GEOLOGICAL SOCIETY OF LONDON
AND AUTHOR OF "ORGANIC REMAINS OF A FORMER WORLD"
HONORARY MEDALLIST OF THE ROYAL HUMANE SOCIETY
POLITICAL AND SOCIAL REFORMER
BORN AT 1 HOXTON SQUARE 11 APRIL 1755
WHERE HE PRACTISED FOR FORTY YEARS
DIED AT 3 PLEASANT ROW HOXTON
21 DECEMBER 1824
BAPTIZED AND MARRIED IN THIS CHURCH
AND BURIED IN THE CHURCHYARD
A LIFE LONG WORSHIPPER AT THE PARISH CHURCH

ERECTED BY THE NURSING STAFF
OF ST LEONARD'S HOSPITAL

Parkinson is best known for his monograph, *The Shaking Palsy* (*Paralysis agitans*) and his descriptions of other medical syndromes such as gout, but he was also a founder member of the Geological Society of London and the author of a formidable three-volume work, *The Organic Remains of a Former World*.

We are indebted to Mr. F. N. L. Poynter, Librarian of the Wellcome Historical Medical Library, for the photograph of the plaque which was unveiled by Dr. A. D. Morris, who has been engaged for many years in preparing a biography of Parkinson.

J. F. F.

The Power of a Postal

It is always pleasant when fellow-historians convened in assembly in foreign countries remember with postcard greetings their American colleagues. This department can announce grateful receipt of a first-day cover from the eminent historian of Fracastoro and of military medicine, General Francesco Pellegrini, in commemoration of the Giornate Mediche Internazionali which were held in Verona in September of 1955. For the occasion the Italian Republic issued a special 25-lire stamp bearing the likeness of Fracastoro and a view of the well-preserved Roman Arena at Verona; the cancellation stamp of the Verona post office for 1 September 1955 was in turn specially lettered in celebration of the meetings.

Later in the same month the Deutsche Vereinigung für Geschichte der Medizin convened at Kassel. A happy reminder of the occasion is a postal initiated by Johannes Steudel and signed by twenty-two other historians, several of them known to American historians not only through their work but also by their presence in September of 1954 at the XIVth International Congress of the History of Medicine in Rome.

A National Air History Museum

In the Annual Report of the Smithsonian Institution for 1954, its general secretary, Dr. Leonard Carmichael, describes the modernization program now under way at the Smithsonian and records the good news that the U. S. Congress, the Fine Arts Commission, and the National Capital Planning Commission are studying plans for a new museum of history and technology, which will include a National Air Museum (occupying an area equal to that of a city block) in which airplane models of historical interest will be exhibited along with airplane engines and equipment. The proposed aviation museum will be similar in scope, but larger in extent than the corresponding Leonardo Museum at Milan. As noted previously (*J. Hist. Med.*, 1955, *10*, 435-436), the Leonardo Museum of Science and Technology also plans to erect a large, new display hangar for aircraft of all types, including models of the ingenious "engines" and flying machines designed by Leonardo.

J. F. F.

Three Centuries of New England Medicine

An exhibit entitled "Three Centuries of New England Medicine" was arranged by Dr. Henry R. Viets, Acting Librarian of the Boston Medical Library, for the regional meeting of the American Medical Association (Ninth Clinical Session) held from 29 November to 2 December 1955. A three-page catalogue was issued, indicating that the exhibit was divided into seven sections as follows:

1. The Colonial Period (1643-1700) with the original of James Thacher's broadside on *Small pocks, or measels,* Boston, 1678, the first medical tract to be printed in the Colonies.

2. The Eighteenth Century to the Revolution (1775) with the literature of the smallpox epidemic of 1721 including Zabdiel Boylston's tracts of 1726 and 1730 and John Walton's *Essay on fevers* of 1732.

3. The Revolution and Its Aftermath (1776-1799) with Stearns' *Almanack* for 1776 and the writings of Drs. Joseph Warren, John Jeffries (the first to cross the English Channel by air), and John Fleet (dissertation of 1795), the first graduate of the Harvard Medical School who was also the first Librarian of the Boston Medical Library.

4. The Nineteenth Century (1800-1846) showing Benjamin Waterhouse on cowpox (1800, 1802, and 1810), Amos Holbrook on vaccination (1810), Elisha North on spotted fever (1811), and the original of Oliver Wendell Holmes on puerperal fever in the short-lived *New England Quarterly Journal of Medicine and Surgery* for 1843.

5. Ether Anesthesia (1845-1848) with William T. G. Morton, C. T. Jackson, Henry Jacob Bigelow, Edward Warren, and Walter Channing.

6. Medicine from 1846-1900. The George Parkman murder (John W. Webster) with Dean O. W. Holmes' testimony; Asiatic cholera epidemic of 1845 and Lemuel Shattuck's *Report* of 1850; Holmes' *Chambered nautilus* (1858); Fitz's *Vermiform appendix* (1886), and Joslin on diabetes (1898).

7. The Modern Period (1900-1955). Mumford on *Medicine in America* (1903), Harvey Cushing on the *Pituitary body* (1912), his *Annual Reports* of the Peter Bent Brigham Hospital (1913-1931), *Osler* (1925), *From a surgeon's journal* (1936), and Vesalius' *Biobibliography* (1943), Joslin's *Manual* (1918), Minot and Murphy on pernicious anemia (1926), Paul Dudley White on *Heart disease* (1931), and James White and Sweet on *Pain* (1955).

J. F. F.

A Post-mortem Discovery in Prague 400 Years Ago

Mrs. Bozena Matousek, D.Sc., has sent to the JOURNAL the reprint of a paper published in a Czech journal (*Zvlástní otisk z Casopisu lékaru ceskych*, 1955, *94*, 631) entitled "A Post-mortem Discovery in Prague 400 Years Ago." The English summary may be of some interest to our readers and is here paraphrased:

In the middle of the sixteenth century official medicine in Prague was repressed to such an extent that even the medical faculty was not functioning, but quite suddenly a group of new physicians and natural scientists made their appearance. The Faculty was revived by a radical upswing of Protestantism and the revolutionary activity of the anti-Hapsburg opposition aroused by the European failures of the Hapsburgs. This is illustrated by a dissection undertaken about ten years after the publication of the work of Vesalius by a Czech physician, Jacobus Camenicenus, and his friends. In spite of the fact that only urinary calculi were known to the classical medical authorities, Camenicenus pointed to the gall bladder of King George of Podebrady which was filled with stones (and was preserved in Prague for nearly a century after being embalmed). At the post-mortem in question Camenicenus himself found stones in the gall bladder and, disregarding the

authority of Galen, he correctly looked for a connection with cirrhosis of the liver and ultimate dropsy of the deceased. He reported the case to the then renowned physician and botanist Mattioli, but from the correspondence included in the collection of Mattioli's letters (*Epistolarum medicinalium libri quinque,* pp. 323-325, published in Prague in 1561) it is clear that the latter failed to grasp the significance of the finding. The discovery of Camenicenus is independent of and contemporaneous with the great work of Fernel in Paris and, even though as an ordinary physician he was unable to secure international recognition for it, it clearly reflects the young scientific movement in Bohemia, again soon suppressed by the Hapsburgs.

J. F. F.

Lectures on the History of Pathology and Experimental Medicine

Readers of the JOURNAL may be interested to know that Dr. Morris C. Leikind, Chief of the Historical Research Division of the Medical Museum, Armed Forces Institute of Pathology, Washington, D. C., will give a series of weekly lectures on Tuesdays at 4:30 p.m. on the history of pathology and experimental medicine. These lectures began on 10 January 1956 and will continue through 10 April and are held in the Dart Auditorium of the Armed Forces Institute of Pathology at the Walter Reed Army Medical Center. All interested are invited to attend.

Dr. Ralph H. Major Lectures in Philippines

Dr. Ralph H. Major has been Visiting Professor and Honorary Head of the Department of Medicine at the College of Medicine, University of the Philippines. From November 5th through February 4th he gave weekly lectures on a wide variety of subjects in the history of medicine from faith healing and temples of healing in Ancient Greece to the rise of American Medicine, and the Johns Hopkins. He also included several lectures on instruments of precision and important books in medicine.

Dr. John F. Fulton's Twenty-fifth Anniversary at Yale

The Editor of this JOURNAL was honored at a celebration held in the Yale Medical Library (Historical Library) on 14 January 1956 at which former research fellows, colleagues, and members of the medical school faculty forgathered to mark the twenty-fifth anniversary of his Yale professorship. Dr. Vernon W. Lippard, Dean of the School of Medicine and Chairman of the Editorial Board of *The Yale Journal of Biology and Medicine,* presented to Dr. Fulton a special number of the Yale Journal (December-February 1955/56, *28,* 161-500) containing thirty-three papers by former fellows and colleagues. As a frontispiece to the issue there is an informal photograph of Dr. Fulton taken by Dr. Gregory Zilboorg in 1953. Hebbel E. Hoff, Professor of Physiology at Baylor University, Houston, who pre-

sided at the dinner given in the School of Medicine the same evening, prefaces the number with an historical account of the Laboratory of Physiology at Yale; this is followed by a bibliography of the 408 books and papers published by Dr. Fulton between 1920 and 1956. The scientific papers begin with Charles D. Aring's interesting contribution on the future of neurology and end with one by Oscar A. M. Wyss of Zurich on "Synchronization of inspiratory motor activity as compared between phrenic and vagus nerves." Dr. John R. Brobeck, Professor of Physiology at the University of Pennsylvania, was editor of this special issue; he was assisted by Frederick G. Kilgour, Managing Editor, and Elizabeth H. Thomson, Assistant Editor.

Dr. Lippard also presented a silver medal engraved as follows: "Physiologist, Teacher, Humanist . . . To celebrate twenty-five years at Yale as Sterling Professor of Physiology and the History of Medicine 1930-1955 from his friends."

E. H. T.
M. E. S.

New Designation for Dental School Museum at Columbia

The Trustees of Columbia University have voted to call the Dental School Museum the "Charles H. Land Museum" in honor of Dr. Land, who was a pioneer in the use of porcelain in dentistry. Dr. Land had the further distinction of being the grandfather of Charles A. Lindbergh. Dr. Curt Proskauer is the Curator of this museum.

American Association of the History of Medicine

The program of the twenty-ninth annual meeting of the Association, which will be held at Durham, North Carolina, on April 19, 20, and 21 as announced in the last JOURNAL, will include two symposia, one on The History of Epidemiology and the other on The History of Medicine in the South, as well as two General Sessions. Participants who wish to present papers are requested to communicate with Dr. George Rosen, Chairman of the Program Committee, Columbia University, School of Public Health and Administrative Medicine, 600 West 168th Street, New York 32. Headquarters will be the Washington Duke Hotel, Durham.

Book Reviews

GEORGE SARTON. *The Appreciation of Ancient and Medieval Science during the Renaissance (1459-1600)*. (The Rosenbach Fellowship in Bibliography.) Philadelphia, University of Pennsylvania Press, 1955. 175 pp., appendix, notes and bibliography, 176-223 pp. $5.00.

Reviewed by DOROTHY STIMSON, *Emeritus Professor of History, Goucher College, Baltimore.*

THE three lectures Dr. Sarton gave as Rosenbach Fellow at the University of Pennsylvania in 1953 were on Medicine, Natural History, and Mathematics and Astronomy. These lectures, much elaborated, form three sections of this book. The fourth, called by Dr. Sarton "Epilogue," is the substance of a lecture he delivered later that spring at the American Philosophical Society, on "The Survival of Ancient and Medieval Men of Science in the Fourteenth and Fifteenth Centuries." As a whole this book is an indispensable addition to Dr. Sarton's monumental *Introduction to the History of Science,* for it brings down to 1600 the great influence of the major figures and some of the minor ones in previous ages upon Renaissance men in the fields indicated. It is *in petto* a substitute for a fourth and a fifth volume of the *Introduction.*

The connecting theme, and the reason for the opening date, is the significance of the printing-press. Not only did the press provide multiple copies with identical texts of any one work, it also made possible multiple copies of illustrations, a fact that Dr. Sarton considers of the greatest importance. Furthermore the early printers, scholars themselves and patrons of scholars, checked and rechecked ancient manuscripts to find exact meanings in a Greek freed from Arabic influences and from mistranslations. Hence in those days scientists also were students of words, philologists: in the process Copernicus turned from Ptolemy to Aristarchos of Samos, and Vesalius studying Galen found it necessary to correct him. Thus this period in the history of medicine, as earlier in art and statecraft, is truly an age of transition from reliance upon the authority of the past to the search for truth in nature rather than in books. For science, the term Renaissance for this age of transition can justifiably be used for the one hundred and fifty years Dr. Sarton has selected, though historians are not likely to agree with his unqualified use of that term in the title of his book.

Except for the Epilogue, this is not a book for general reading but a valuable work of reference, rich with the work of men little known today, with translations, editions, dates. The Contents lists eight or more subdivisions for each of the three major sections and the Index provides the key to all names mentioned. The Notes are "Sartonian" in their number and erudition. One disconcerting misprint (p. 168) should be corrected in

the next edition: "a type of alchemist and occulist . . . in the sixteenth century . . ." surely should be "occultist."

It is indeed fortunate for all students of the history of science, as well as for others studying these centuries, that the dean of the historians of science in this country has been able to give permanent form to at least some of the material he had amassed for the continuation of his *Introduction.*

WHITFIELD J. BELL, JR. *Early American Science. Needs and Opportunities for Study.* Williamsburg, Virginia, Institute of Early American History and Culture, 1955. vii, 85 pp. $1.25.

Reviewed by the EDITOR.

THIS beautifully printed survey is the first to result from a series of conferences during 1952-1953 on "Early American Science, Early American Law, and Indian Relations Prior to the Removal Act of 1830," sponsored by the Institute and the College of William and Mary at Williamsburg. In a foreword by Lyman H. Butterfield, then Director of the Institute, the spirit of eighteenth century Americans engaged in scientific investigation is illustrated by a stimulating quotation from the Reverend Jeremy Belknap's *History of New Hampshire* (1792) which runs: "The book of nature is always open to our view, and we may study it at our leisure . . . The earth, the air, the sea, the rivers, the mountains, the rocks, the caverns, the animal and vegetable tribes are fraught with instruction. Nature is not half explored; and in what is partly known there are many mysteries, which time, observation and experience must unfold."

Mr. Bell, formerly Professor of American History at Dickinson College and one-time Editor of the *William and Mary Quarterly,* is now Assistant Editor of the Papers of Benjamin Franklin which are being published under the joint auspices of the American Philosophical Society and Yale University. Following the text of the survey Mr. Bell appends a bibliography divided into four parts which will be indispensable for any historian who concerns himself with American culture during Colonial times. Part I lists general histories of science, bibliographies, etc., Part II, books and papers dealing with the history of science in America up to 1820; Part III gives a useful list of periodicals devoted to the history of science, Part IV, the names and brief biobibliographies of fifty early American scientists, again up to about 1820.

Readers of this JOURNAL will also find the book useful for its references to medical education and to early contributors, including Cotton Mather, who concerned themselves with medicine. The early Colonial was of a practical mind and his interest in science often stemmed from a desire to make productive applications in fields such as astronomy and navigation. In this connection attention is directed to Thomas Jefferson's report of 1790 on cod and whale fisheries, and John Quincy Adams' discourse of 1821 on weights and measures.

This valuable study comes well up to the high standard of scholarship and what Mr. Butterfield has so aptly called "spirited writing" which we have come to expect from Mr. Bell. The Institute of Early American History and Culture is to be congratulated on this new addition to their growing list of timely and useful publications.

L. FABIAN HIRST. *The Conquest of Plague: A Study of the Evolution of Epidemiology*. Oxford, Clarendon Press, 1953. xvi, 478 pp., illus. $11.00.

Reviewed by ILZA VEITH, *Assistant Professor of the History of Medicine, The University of Chicago, Illinois.*

THIS excellent book is a progress report on the conquest of the plague, rather than a final chapter on the subject. As the author himself states, the plague is by no means entirely conquered, for ". . . we have as yet to discover effective ways of extirpating the seeds of plague dormant from time immemorial among the wild rodents of remote regions of Asia. Since the world pandemic of our times began its spread in 1894, vast and intractable new reservoirs of campestral rodent plague have also been established in Africa and America." And yet, in spite of these reservations, plague has ceased to be the dread disease it once was, for owing to the recent epidemiological discoveries mankind has learned to protect itself against this ancient scourge and to cure most of its victims.

Dr. Hirst makes these statements with eminent authority, for he can look back on many years of active concern with the plague. He began his studies in London in 1910 and continued them for nearly a quarter of a century in the Middle and Far East. In 1912 he joined in the work of the newly built microbiological laboratory at Colombo in Ceylon; it was there also where many of his writings on the plague were published. Even when illness terminated his laboratory work in 1934, his preoccupation with the plague continued and led him to the Bodleian Library and the Library of the British Museum. Thus *The Conquest of Plague* is the combined result of his active work on the plague and his subsequent studies of the literature on the subject.

The book is divided into four major parts and several appendixes containing purely technical information, which the author separated from the body of his work so as to make it understandable and interesting to as wide an audience as possible.

In presenting the first part, entitled "Traditional Conceptions of the Nature of the Plague," Dr. Hirst states significantly that "only in an historical setting can modern epidemiological ideas be understood: only when the subject is viewed from the historical standpoint can we realize the magnitude both of our debt to the pioneers of the past and of the task which still requires to be accomplished." Although this historical survey is largely based on standard medico-historical writings on the plague, such as those of Sticker, Creighton, Singer, *et al.*, it is an immensely valuable summary

of the subject. The other three parts deal with "Modern Conceptions of the Nature of the Plague," "The Spread of Plague in Time and Space," and finally, with the theme of the work, "The Conquest of the Plague" itself, referring to the international and national controls, personal protection against infection, and the modern treatment of the disease.

The book has the happy combination of being informative, authoritative, and very readable. It is particularly valuable for the medical historian in providing him with the technical and scientific background of a disease whose far-reaching influence on the history of mankind can never be fully gauged.

BERN DIBNER. *Heralds of Science as represented by two hundred epochal books and pamphlets selected from the Burndy Library.* Norwalk, Connecticut, 1955. 96 pp. $2.00.

Reviewed by MARSHALL CLAGETT, *Associate Professor of the History of Science, The University of Wisconsin, Madison, Wisconsin.*

HISTORIANS of science and medicine will welcome this handsome volume describing selected books and pamphlets from the Burndy Library in Norwalk, Connecticut. As the author tells us in the preface, the volume "is an effort to select, no matter how imperfectly, a comprehensive number of books representative of the most important contributions to the physical and biological sciences issued since the beginning of printing." The format and general make-up of the work are, like Mr. Dibner's preceding publications, excellent. The illustrations chosen for many of the volumes are particularly noteworthy. They themselves represent a phase of the history of science seldom studied.

The divisions adopted in this volume are by subject matter and are naturally somewhat arbitrary: Astronomy, Botany, Chemistry, Electricity and Magnetism, General Science, Geology, Mathematics, Medicine, Physics, Technology, and Zoology.

The notes describing each of the 200 items are for the most part accurate and informative. Not everyone would agree that the particular volumes selected are the most significant, but presumably the selection was in some ways restricted by the limits of the collection. Historians of medicine will be interested to know that the section on medicine contains some nineteen items ranging from Hippocrates and Galen through Vesalius, Harvey, Morgagni, Lind, Beaumont, and others to Pavlov and Ehrlich.

It is hoped that this fine volume will draw students' attention to the Burndy Library and its collection which contains, we are told in the introduction, some 5,000 volumes on electricity and magnetism, some 200 incunabula, a special Leonardo collection of 800 volumes, about 1,000 letters and manuscripts, and some 2,000 separate portraits of scientists and engineers.

PAUL F. RUSSELL. *Man's Mastery of Malaria.* (University of London Heath Clark Lectures 1953.) London, New York, Toronto, Oxford University Press, 1955, xiv, 308 pp., 21 illus., $6.00.

Reviewed by ERWIN H. ACKERKNECHT, *Professor of the History of Medicine, The University of Wisconsin Medical School, Madison, Wisconsin.*

MANY leading malariologists, from Ross to Celli to Barber, Boyd, and Hackett, have made outstanding contributions to the history of malaria and with his fine Biggs lecture of 1953, Dr. Russell joins this succession. His book is not a systematic history of malaria; earlier developments are rather selectively reported from the extensive literature on the subject. More than half of the book is dedicated to the timely and very worthwhile task of evaluating the dramatic and unprecedented progress of the last two decades. The history of our knowledge of aetiology is reviewed from supernaturalistic theories to the recent discoveries of the exoerythrocytic phase. The history of treatment is given with perhaps too much indulgence for the bloodletting-calomel era and with emphasis on the development of the modern synthetic drugs. The history of prophylaxis is dominated by a report of the incredible advances brought about by DDT spraying of houses. The now recorded near disappearance of malaria from Italy, Greece, or Mauritius, or drastic reduction in India would have been regarded as utopian no more than ten years ago. International coöperative efforts against malaria are surveyed interestingly. A very forthright and clear last chapter deals with the panicky reactions of some public health men who would like to stop antimalaria work in certain countries because it increases population pressures.

It seems, in general, unwarranted to argue about omissions with a book intended to give only highlights. I nevertheless feel that screening and its history would, by any criteria, have deserved more attention. Why in the face of his own evidence the author accepts Crombie's entirely unfounded verdict about the "striking results" of medicine in the middle ages, or how he learned that Paracelsus attempted to isolate the active principle of cinchona, remain mysteries to me.

These minor strictures do not detract from the value of this well-written and well-documented book which is a must for all interested in the history of malaria.

ADAM PATRICK. *The Enteric Fevers 1800-1920.* Edinburgh, Royal College of Physicians of Edinburgh Publications, 1955. 46 pp.

Reviewed by JOSEPH L. MELNICK, *Professor of Epidemiology, Yale University School of Medicine.*

IN the Sydney Watson Smith Lecture for the year 1954, the author presents a brief account of the changing concept of enteric fevers during the nineteenth and first part of the twentieth century. The development of the recognition of typhus and typhoid as distinct entities is traced from the

writings of the first investigators in the field. After 1850, emphasis changed from symptomatology and pathology to epidemics and their causes, and Patrick discusses the state of hygiene at the time and the low opinion in which physicians held the endeavors of the early sanitary authorities. Selected quotations from the writings of William Budd are given, including those pertaining to his controversy with Miss Florence Nightingale about spontaneous generation of disease. His clear arguments on the spread of typhoid by the excreta of patients, especially on the "swallowed infection" (milk-borne epidemics), did not cause this great lady to alter her opinions.

The prevalence of typhoid fever in Great Britain and the resultant mortality are briefly discussed, as are the discovery of the causative bacillus by Eberth in 1880, the agglutination test for the typhoid organism or its antiserum (Widal test), and the realization that intestinal carriers may be sources of infection for years. The events leading to a vaccine made from killed organisms are briefly described, together with the field trial of the vaccine in troops and the complications involved until the differential diagnosis of typhoid and paratyphoid became clearly established.

One looks forward to an account, in Patrick's lively style, of the enteric fevers from 1920 on, which would take us into the viral field. A number of viruses have been isolated in recent years from human feces: poliomyelitis, hepatitis, Coxsackie, and orphan viruses. Through the use of new tissue culture techniques the number of viral agents being isolated from the intestines steadily increases. To what extent they might be involved in enteric fevers remains a problem for future investigations.

DANIEL PAUL SCHREBER. *Memoirs of My Nervous Illness.* Translated, edited, with introduction, notes and discussion by Ida Macalpine and Richard A. Hunter. London, Wm. Dawson & Sons, 1955. 416 pp.

Reviewed by HENRY R. VIETS, *Acting Director, Boston Medical Library, Boston, Massachusetts.*

DANIEL Paul Schreber, a doctor of jurisprudence, born in 1808 of a distinguished family, suffered twice at the height of his career as a judge at the Country Court from "nervous illness." The first attack lasted about a year and was described as a severe hypochondriasis. He was treated by Professor Paul Emil Flechsig at the Psychiatric Clinic of the University of Leipzig and was considered as fully recovered when, in 1885, he resumed his position. He was later made President of the Court of Appeal at Dresden, but six weeks after taking up his duties he again became ill, this time with a more pronounced psychosis requiring hospitalization for nine years, also under Professor Flechsig's care. It was during this period that he kept extensive notes of his symptoms and after he left the Clinic these were fashioned into a book, *Denkwürdigkeiten eines Nervenkranken,* published in Leipzig in 1903. The original edition has become excessively rare, partly because the Schreber family is said to have bought up and destroyed most

of the copies of the Memoirs. The book has now been translated into English by Mrs. Ida Macalpine, an M.D. from Erlangen in 1926 now at Saint Bartholomew's Hospital, and Richard Alfred Hunter (M.D. University of London, 1952), associated with Guy's Hospital, London.

The translation was not an easy task because of Schreber's circumstantial, complicated, and cumbersome presentation of psychotic trends of thought. So manifold were his symptoms that almost the entire field of psychiatric abnormality is described. Some of his attempts at giving an account of his ideas and feelings are literally not expressible in human language, yet the authors have succeeded in reproducing the import of Schreber's psychosis, as described in his own language, in a realistic manner and most of their translation is understandable.

Schreber's book is widely quoted in modern psychiatric and psychoanalytic literature. By many it is considered the best text on psychiatry written for psychiatrists by a patient. It was favorably reviewed when it appeared in 1903 and has since received abundant recognition in all textbooks. Freud found it an "invaluable book" and others have praised it highly. Most authors place Schreber's illness under the heading of paranoia or schizophrenia and almost invariably write about it in connection with Freud's name. Quotations are usually from Freud's paper; the book itself seems to be seldom consulted, partly because it is rarely found in medical libraries. To rectify this situation, Macalpine and Hunter have gone back to the original publication. They have opened a rich mine, not previously fully explored, for Schreber offers unique insight into the mind of a schizophrenic, "his thinking, language, behaviour, delusions and hallucinations, and into the inner development, course and outcome of the illness. His autobiography has the advantage of being complete to an extent no case history taken by a physician can ever be."

The translation covers 328 pages. There are critical notes, a list of references, but no index. The title-page of the original edition is reproduced, with a portrait of Flechsig and photographs of the Sonnenstein Asylum, where Schreber was confined. Three extensive phychiatric reports, written by the leading medico-legal psychiatrists of the day, are appended to the Memoirs. These will be of interest to lawyers as well as physicians. For rescuing the "Schreber Case" from its obscurity, historians, psychiatrists, and others are gratefully indebted to the editors for a trying task well done.

SIGMUND FREUD. *The Interpretation of Dreams.* Translated from the German and edited by James Strachey. New York, Basic Books, Inc., 1955. $7.50.

Reviewed by JACQUES M. QUEN, *Assistant Resident in Psychiatry, Veterans Administration Hospital, West Haven, Connecticut.*

"*The Interpretation of Dreams* . . . was finished in all essentials at the beginning of 1896 but was not committed to paper until the summer of 1899." So says Sigmund Freud in *The History of the Psychoanalytic Movement* (1914). The first edition of *Die Traumdeutung,* though dated 1900, was in

Freud's hands by November 1899. By 1930 it had gone through eight editions and seven revisions by the author. A. A. Brill did the first English translation in 1913; this had undergone but two revisions by 1955. At first glance it is surprising that this book—critical not only in the development of psychoanalytic but in the development of all the ancillary psychological sciences—should not have produced more attempts at a definitive translation. However, when one studies the formidable labor involved in any such attempt, it becomes understandable. Freud in his numerous revisions deleted, added, and revised without adequate annotation and reference. The German editions contain frequent inadequate and occasionally puzzling references. The bibliographies were on occasion woefully inaccurate as well as replete with concepts and words difficult to translate into English.

Working in close collaboration with Miss Anna Freud, Mr. Strachey has accomplished what strikes this reviewer as a truly Herculean task in a manner Herculean. When one considers what Mr. Strachey has done in 692 pages, it becomes apparent that this English translation is far superior to any single edition of the original work. He has managed to combine all eight editions in one volume, marking additions, deletions, and revisions made by Freud and dating them. Also he has made annotations with references (again dated) to Freud's other works in relation to specific terms and spheres of interest. He has enlarged and revised the bibliographies, making them more accurate than in the original. He has provided a general index of some twenty-five pages as well as an index of dreams, distinguishing between those dreamt by Freud and those dreamt by others. In addition to all this Mr. Strachey writes in a style which is lucid, clear, and quite readable.

He has translated *Unlust* as "unpleasure," providing a neologism where one was sorely needed, and he breaks with tradition where he feels it necessary, such as abandoning the phrase "secondary elaboration" in favor of "secondary revision." One feels that the excellence of this scholarly work strongly argues in favor of the adoption of such innovations.

This edition should be of special interest to readers of this JOURNAL since it provides a ready and reliable reference to the historical development of much of the psychoanalytic movement and of the changes and variations in Freud's views of many aspects of psychoanalysis from 1899 through 1930.

ERICH AUER. *Entwicklung und Stand der sozialmedizinischen Reform in der Schweizerischen Industrie. Ein Beitrag zur Geschichte der Arbeitshygiene.* (Basler Veröffentlichengen zur Geschichte der Medizin und der Biologie, Fasc. V.) Basel, Benno Schwabe & Co., 1955. 76 pp., illus. 950 sw. fr.

Reviewed by GEORGE ROSEN, *Professor and Head of the Department of Public Health Education, Columbia University, New York City.*

MAN is a social being. Men act upon each other and enter into relations with each other, thus creating the framework within which social activity proceeds. Consequently, sick people are always located within a particular

space-time setting under definite societal conditions. Insofar, then, as disease arises from or affects the social conditions under which men live and work, it must be seen as a social phenomenon and is completely comprehensible only within its social context. This recognition of the social nature of disease focuses attention upon areas of study somewhat removed from the traditional problems of medical history. One of these areas is occupational health, where one must take account not only of medicine but of technology and social conditions as well.

Dr. Auer makes an interesting contribution to the history of occupational health by tracing the development of industrial hygiene, especially its legal aspects, in Switzerland. While an awareness of occupational hazards is found before the nineteenth century, it was not until the latter half of the century that legal action was taken to protect the health of adult workers. In 1864, the cantons of Glarus issued a factory inspection law which included adults and limited the normal working day to twelve hours. Night work was prohibited and children under thirteen years of age could not be employed. A factory inspectorate was created, and it was in his capacity as inspector that the physician Fridolin Schuler developed measures for the protection of workers and became the founder of this branch of social medicine in Switzerland.

From this beginning the author shows how a body of legislation developed for Switzerland and was applied with ever-increasing effectiveness. This monograph is the fifth volume to appear in the series of *Basler Veröffentlichengen* recently initiated by Dr. H. Buess, and it is a worthy addition. It will be of interest to anyone concerned with the history of occupational health and social medicine.

LUCY WILDER. *The Mayo Clinic.* Illustrated by Ruth Barney. 2d Ed. Springfield, Ill., Charles C Thomas, 1955. 70 pp. $3.75.

Reviewed by ROBERT ROSENTHAL, *Clinical Assistant Professor of Pediatrics, University of Minnesota School of Medicine, Minneapolis, Minnesota.*

ALTHOUGH the popularity of *The Mayo Clinic* necessitated several reprintings, this is the first new edition since it appeared originally in 1936. Most of it is identical with the first edition, but the important developments have been brought up to date.

This thin volume with its concise text has a different scope than Helen Clapesattle's *The Doctors Mayo.* It was written, as is stated on the jacket, for visitors to Rochester who desire information about the Clinic. This task has been excellently performed, in fact, so well that the book gives a short but well-rounded and satisfactory story even for a medical student or practitioner who wishes to learn about the Clinic.

The first 21 pages deal with "The Past." They give a good account from the time when William Worral Mayo, a young chemist, left England and came to the United States. In swift strokes the author leads us through young Mayo's medical studies and his peregrinations until he settled in Rochester, Minnesota in 1863. The romantic story of Dr. W. W. Mayo, of his famous sons, William James and Charles Horace Mayo, and of the development of the Clinic is admirably told in a short space; all important information can be found in it, from the first coöperative venture of the Mayos with St. Mary's Hospital. The writer tells of the growth of the Mayos' practice and of the hospitals, the enlargement of the staff, and the widening of the scope of their Clinic from almost a purely surgical one to an all-round medical and research institution. The development of the Mayo Foundation for Medical Education and Research, which was established in 1915 and eventually became a department of the Graduate School of the University of Minnesota, is well described.

The second portion of the book, designated "The Future," deals with the development of the Mayo Clinic as a teaching institution, the broadening of the scope of the Clinic during the past fifty years, and the development of the various sections, including the library and the publication and social services, etc.

The last portion is devoted to the routine procedures at the Clinic from the moment the patient enters until he is discharged. It tells all about the registration, the routine tests, the consultations, etc., which lead up to the final diagnosis and advice or treatment. This portion is defintely meant for lay readers. The rest of the book can be read to advantage by medical men who are interested in the Mayo Clinic.

The edition retains the letter of Dr. William J. Mayo written in 1934 to the University of Minnesota when the brothers added a half million dollars to the endowment of the Mayo Foundation. It is indeed—to use the late President L. D. Coffman's words—a sincere document, mature in philosophy, and unpretentious.

The frontispiece of the first edition, which pictured the Clinic building opened in 1929, has been replaced by a view of the new diagnostic building, completed in 1954. A colored illustration of the historical window in the Mayo Foundation House, showing the many phases in the development of medicine, is new. The well-known photograph of Dr. William Worral Mayo and his two sons has been replaced by one showing James Earle Fraser's bronze of the two famous brothers. The marginal drawings by Ruth Barney have been retained. It is the reviewer's opinion that they do not add to the dignity of the book. The book is marked by the fine printing and design that characterized the first edition.

ELMER BELT. *Leonardo the Anatomist.* University of Kansas Press, Lawrence, Kansas, 1955. $2.00.

Reviewed by H. S. BURR, *E. K. Hunt Professor of Anatomy, Yale University School of Medicine.*

LEONARDO da Vinci, one of the great minds of all times, is always a romantic figure. It is not surprising, therefore, that the Fourth Series of the Logan Clendening Lectures on the History and Philosophy of Medicine should be devoted to Leonardo the Anatomist. The author, Dr. Elmer Belt, has assembled the largest collection of material about Leonardo in private possession.

In this lecture Dr. Belt presents, in what one must confess is a somewhat pedestrian manner, the anatomical studies of the great artist. Without much question Leonardo was the first great anatomist who, unfortunately for posterity, was not particularly concerned with rushing into print. Many years elapsed, therefore, between his death and the first reproduction of his work. Two curious misstatements occur on pages 34 and 35, which must have been unintentional slips. For example, the trochlear nerve is the fourth cranial nerve, not the third, and apparently was not described by Leonardo. Also the ulnar nerve supplies the lateral aspect of the hand, and the median nerve the medial portion, as its name would imply, and not as stated on page 35.

One paragraph on page 66 deserves to be quoted in its entirety: "Leonardo was the first artist who is known to have advanced beyond the anatomy of flayed bodies and to have carried his studies into the deeper muscles and thoracic and abdominal viscera. Fortune combined in him the abilities of a graphic artist of the greatest powers, an infinite scientific curiosity, and a phenomenal ability to analyze his experiences. He developed through these powers an understanding of natural forces far beyond others of the stirring age in which he lived."

REUBEN FRIEDMAN. *A History of Dermatology in Philadelphia.* Fort Pierce Beach, Florida, Froben Press, 1955. 556 pp., illus. $10.00.

Reviewed by JACK J. ALBOM, *Associate Clinical Professor of Dermatology, Yale University School of Medicine.*

THE author, a practising dermatologist for many years, has essentially limited his interest in dermatological history to Philadelphia and to the dermatologists who made this city the great training center that it is today for that specialty.

Several chapters concern themselves with European dermatologists: Willan (English), Alibert (French), and Hebra (Austrian), and their impact on Duhring, the father of Philadelphia dermatology.

The author divides dermatology in Philadelphia into three eras:

1. The era before Duhring, when no dermatological progress had been made.

2. The era of Duhring (1870-1910), when dermatology was accepted as a specialty in American medicine and when Duhring, at the age of twenty-five years, embarked on his specialty and brought international fame to himself and the University of Pennsylvania, with which institution he was closely associated throughout his career. Dermatologists associated with and trained by Duhring, such as Van Harlingen, Stelwagon, Hartzell, and Schamberg are ably sketched by medical colleagues who knew them intimately.

3. The era after Duhring deals primarily with dermatology at the University of Pennsylvania under Stokes and Pillsbury and at Temple University School of Medicine, Jefferson Medical College, and the Hahnemann Medical College. There are interesting accounts of the Research Institute of Cutaneous Medicine, the Skin and Cancer Hospital, and the Laboratory of Dermatologic Research under the guidance of the renowned dermatopathologist Weidman.

This book will be of limited interest because of its restricted subject matter. Moreover, it is not remarkable for its continuity. However, to the dermatologist it presents realistic character studies of men whose names are well known in this specialty. And especially to the dermatologist who has been trained in Philadelphia, it will bring back memories of worthwhile medical associations.

RICHARD M. BURKE. *An Historical Chronology of Tuberculosis.* 2d ed. Springfield, Illinois, Charles C Thomas, 1955. xiii, 125 pp., 6 illus. $3.75.

Reviewed by the EDITOR.

THE history of tuberculosis is a matter of vital interest to all men of medicine, and the present chronology by Dr. Richard M. Burke, Assistant Professor of Medicine at the University of Oklahoma School of Medicine, now appearing in its second edition, will be a useful tool for the finding of dates and of important personalities concerned with the disease. In Chapter 1 the ancient period, 5000 B.C. to 1600 A.D., is briefly outlined and correlated with events of general history. Chapter 2 deals with the pre-modern period from 1600 to 1800 and the impact of anatomy and physiology upon the study of the disease. Chapter 3 has to do with the more recent period of 1800 to 1881 when the techniques of morbid pathology were being put to use. The final and fourth chapter deals with the modern period from 1882 to the present time when the science of bacteriology had come to the fore. There is an excellent bibliography and an index of names.

STEPHEN FINNEY MASON. *Main Currents of Scientific Thought: A History of the Sciences.* New York, Henry Schuman, 1953. viii + 513 pp. $5.00.

Reviewed by FREDERICK G. KILGOUR, *Librarian, Yale Medical Library.*

Main Currents of Scientific Thought is a general history of science from Babylonia and Egypt to the mid-twentieth century. It has forty-eight chapters averaging a little over ten pages each. There are no illustrations, a circumstance which detracts from the value of the book.

Although the author has made some effort to integrate the history of science with general history instead of writing the usual internal history of science, the book is inferior to other works such as W. C. Dampier's *A History of Science,* Charles Singer's *A Short History of Science to the Nineteenth Century,* and W. P. D. Wightman's *The Growth of Scientific Ideas* despite the fact that the last two do not cover the twentieth century. Some sections of the book have so many inaccuracies as to make it inadvisable to use it as a text for students and necessary to caution any reader. For instance, it can be pointed out from one paragraph on page 176 that Fabricius did not spell his name "Fabricus"as it occurs twice in this paragraph; that it was not "In 1603 [that] Fabricus made the important discovery that there were valves in the veins"; that Harvey did not argue "it was unlikely that blood would be continuously prepared at the ends of the veins and continuously destroyed at the ends of the arteries"; that Harvey did not calculate "the amount of blood passing through the heart in the space of an hour"; and that he did not show such an amount "to exceed the whole weight of an average man." This is too much error for one paragraph.

J. MENZIES CAMPBELL. *Dentistry as Practised 1800-1921.* A pamphlet sold in aid of the Benevolent Fund of the British Dental Association, 13, Hill Street, Berkeley Square, London W.1. 11 pp. 2/6.

Reviewed by the EDITOR.

THIS pamphlet, reprinted from three articles that appeared originally in *The Glasgow Herald* is divided into three parts covering the periods respectively 1800-1850; 1850-1878; 1878-1921. The author records that despite the efforts of John Hunter, dental practice in 1800 was on a lower plane than any other branch of the healing arts; during the second period various technical improvements were introduced, but even the introduction of antisepsis by Lister in 1865 was not recognized until much later by the dental profession; the year 1878 is that of the Dentists' Act and the first Dentists' Register which did much to improve the condition of the profession both in Glasgow and elsewhere.

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